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Hans Jenny

SOIL SCIENTIST, TEACHER, AND SCHOLAR

With Introductions by
Arnold M. Schultz
Allison Warner

Interviews Conducted by
Douglas Maher
in 1979, 1980, 1983
and
Kevin Stuart
in 1983

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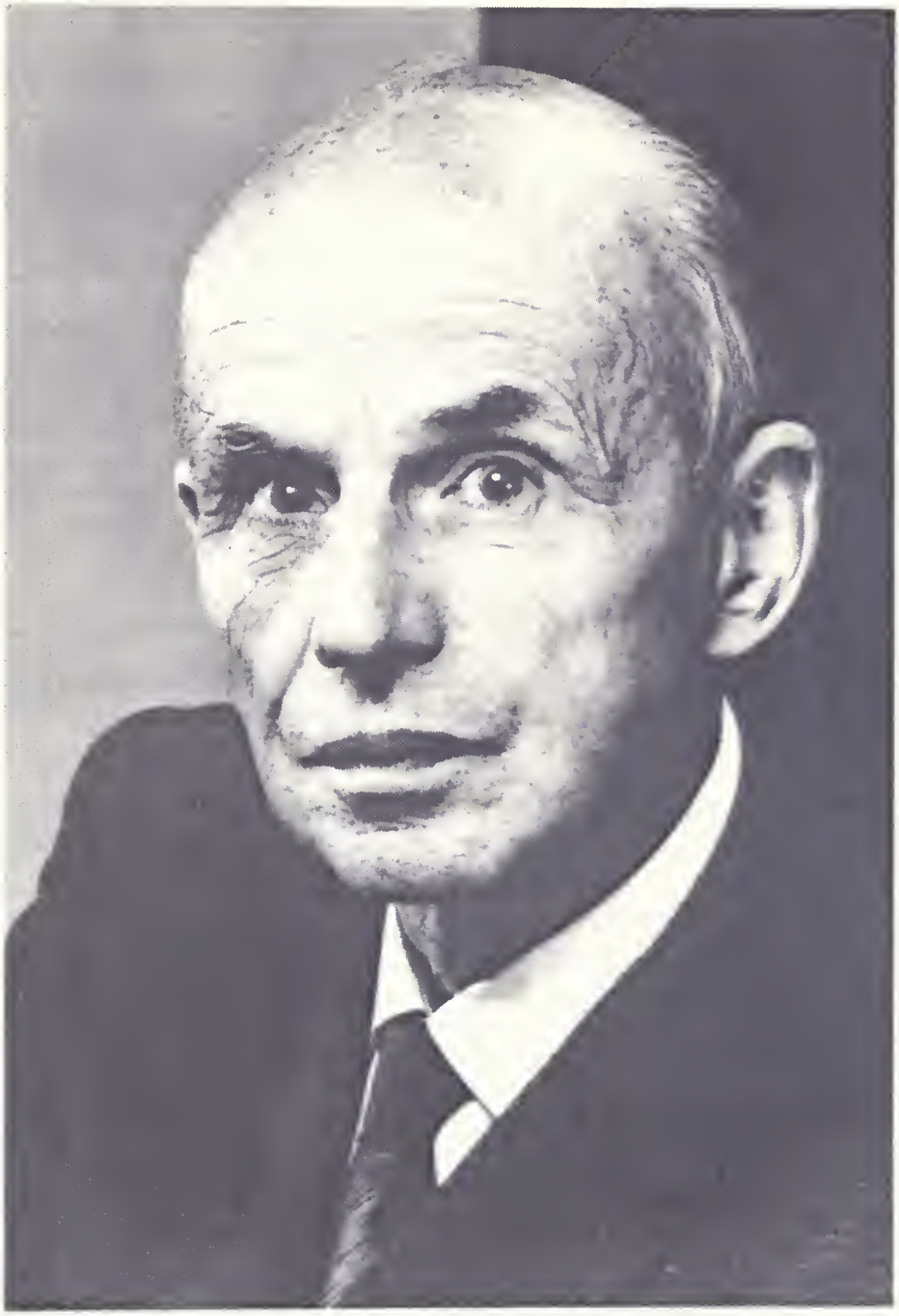

HANS JENNY
1967

Photograph by Paul Bishop

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Boyhood and early education in Basel, Switzerland; graduate study in colloidal chemistry, Swiss Institute of Technology, Zurich, 1919-1925; lab and field work, New Jersey College of Agriculture, 1926-1927; teaching and research, University of Missouri, 1927-1936, and University of California, Berkeley, 1936-1967. Influences of Georg Wiegner, Albert Einstein, Robert Bradfield, S.A. Waksman, and Eugene Hilgard. Soil Conservation Service and leaders in soil science: C.E. Marshall, Curtis F. Marbut, Emil Truog, Charles E. Kellogg, Guy Smith. Pioneering use of ultramicroscope, atomic and molecular research; aesthetics of soil; preservation of natural soils; pygmy forest research. Writing Factors of Soil Formation; biography of Eugene Hilgard.

Introductions by Arnold M. Schultz, Professor of Forestry and Conservation, and Allison Warner, graduate assistant, Department of Forestry and Resource Management.

Interviewed 1978-1983 by Douglas Maher and 1983 by Kevin Stuart. The Regional Oral History Office, The Bancroft Library, University of California, Berkeley.

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INTRODUCTION BY ARNOLD M. SCHULTZ

It is an honor to be writing the introduction to the oral history of a great man, scientist and humanist, one who has dedicated his life to the study and care of earth as soil and to the earth as ecosystem. I have known him personally for forty years and through his work for ten years before that. Still today, in his ninetieth year, we are working together on a project that we started twenty-five years ago. For me, the privilege of working with Hans Jenny has been like receiving a medal.

I first heard of H. Jenny in 1941 when I was taking my master's degree in Plant Ecology with W.S. Cooper at the University of Minnesota. A number of Jenny's early Swiss papers were cited in Fuller and Conard's translation of Braun-Blanquet's Pflanzensoziologie. After World War II I went to the University of Nebraska for a Ph.D. in Botany with the eminent ecologist J.E. Weaver. My minor field was Soil Science. A soil chemistry course introduced me to Jenny's contact theory and to his paper on colloid chemistry and ion exchange. I wasn't impressed with these papers because I didn't understand them very well. Then I took another soils course from Dr. H.F. Rhoades who suggested a book, Factors of Soil Formation, as supplementary reading; for me it became the text! I thought it was a wonderful ecology text even though the word ecology appeared only once, and not until page 263, six pages from the end of the book. After finishing my degree, Jim Thorpe, a USDA soil scientist, offered me a good job but by then I had had three years of digging in the soil, digging out grass root systems for Dr. Weaver, so working in vert space appealed to me more. I took a position at Berkeley in the School of Forestry. Soon after coming to Berkeley in 1949 I met Professor Jenny in person. It didn't take very long and I was digging in the soil again--this time to excavate Ponderosa pine tree roots which go much deeper than grass roots. The awe in which I held Professor Jenny subsided here--there's nothing like digging in the soil and sweating alongside another person to break an awe-barrier. During this project, Professor Jenny became Hans and since then, for me, he has always been Hans.

During the early 1950s Bay Area ecologists (all academics--the activist kind had not yet been activated) held stimulating seminars on the Berkeley campus about once a month. They were attended by such people as Ledyard Stebbins, Herbert Mason, Carl Sauer, Frank Pitelka, and professors from neighboring institutions. Even Henry Gleason came once in a while. Papers were presented semi-formally. One of the purposes the seminar served was the opportunity for authors to get pre-publication critique from colleagues. And critique was always intense and sharp. Except once. Hans Jenny presented the paper which he later published in Ecology on the biotic factor. I recall that there was almost no comment. The presentation was impeccable and airtight. No one could offer a counter argument or suggest

improvement. It has always intrigued me how Hans writes and lectures: rich in ideas, frugal with words--like a landscape painter who uses only a few simple lines.

I once said to him, "Hans, I've always thought of you as a soil scientist, but now I realize you really are an ecologist."

"No," he demurred, "I don't know much about ecology but about ecosystems I know a lot." I then realized the real difference between ecology and the study of ecosystems. Soon thereafter I started a graduate seminar, and later an undergraduate course, called Ecosystemology, each being quite different from the ecology course that I had been teaching at the same time. Incidentally, the term 'ecosystem' was coined by the British ecologist, A.G. Tansley, and appeared first in print in 1935 in the American journal Ecology. While writing Factors of Soil Formation (1941), Hans Jenny had not been especially conversant with the ecological literature and was not aware of the term 'ecosystem'. However, the concept of ecosystem was most clear in his mind as he realized that the factors which determine soils are also the factors that determine "the larger system"--his own coinage.

Hans and I have discussed both the conceptual and pragmatic aspects of ecosystems over these past forty years, so most of our differences have been ironed out. Early on--I think it was in 1954--Hans and I organized an ecosystem colloquium. We invited Bob Whittaker, John Curtis, Roger Bray, Evell Gorham, Elfyn Hughes (from Bangor, Wales) and about twenty others. We tried to get Ludwig von Bertalanffy, the Austrian biologist who developed General Systems Theory but he eluded our invitations. There were no papers, only discussion, and twenty-five different viewpoints on ecosystems! Thirty years later we held another ecosystem colloquium, this time with Wes Jackson of the Land Institute in Salina, Kansas and Stanley Rowe from the University of Saskatchewan in Saskatoon. We held it at the Jenny farmhouse in Mendocino County, near where we had some success at preserving the Pygmy Forest. Some prominent ecologists had announced that the ecosystem concept proved to be useless and should be laid to rest. The purpose of our foursome was also preservation: to preserve the concept and show that its use was essential in studying the Pygmy Forest.

Hans Jenny is exceedingly thorough in everything he does; he gets the most out of every effort. While he was writing his last book, The Soil Resource, he spent many weeks studying the mathematics of multivariate analyses. Ultimately little of this effort was used explicitly in the book, but he felt that he needed to understand it thoroughly before deciding whether or not to use it. An anecdote that further illustrates this custom of thoroughness must be told. From 1958 to 1965 I was working on an Arctic ecosystem project funded by NSF; it was headquartered at Point Barrow, Alaska. In my budget I had put an item which permitted me to bring an "idea-person" as consultant for several weeks every year. In 1960 I invited Hans. He had never been to the Arctic tundra before, and in preparation for his first visit he studied up on it with the intensity of a graduate student preparing for the orals. Among other things Hans learned

that the annual precipitation at Barrow, Alaska was four inches--a desert. We flew up to Barrow together in late June. Snow had already melted but the Arctic lakes were still frozen, producing a distinctive landscape of elliptical white patches. As we flew across the Brooks Range and suddenly saw the level plain obliquely ahead, Hans exclaimed, as if he expected it to be coming, "Aha! alkali flats!"

The "idea-man" idea paid off with Hans' visit to the tundra. After observing some of my field plots, he quickly noted that the year-to-year variability in depth of thaw (i.e., thickness of the active soil layer above permafrost) was correlated with the lemming cycle and not with yearly solar radiation as the geophysicists thought. Data from subsequent years verified this discovery. Until now, I have not given Hans credit for pointing it out.

The Soil Resource is organized like Hans Jenny's professional life. The first half of the book (Part A) deals with soils on a molecular and ionic level--essentially a reductionist approach. Through the early part of his academic life Professor Jenny was primarily concerned with soil chemistry. I believe that a chronological listing of his papers would bear this out. (But by no means was all his work done in laboratories. Broader conceptual schemes arose from visits to the Alps, Colombia, Egypt, India, and Kilimanjaro.) The second half of the book (Part B) is holistic, dealing with ecosystems and whole landscapes. The latter part of his life, especially since retirement, Hans Jenny has devoted largely to the preservation of representative segments of soil types, of rare and endangered ecosystems, and unique landscapes so that they can be enjoyed and yet further studied by generations in the future. More than anyone else, he has been responsible for recognizing and publicizing the scientific significance of the Jenny-made-famous Pygmy Forest on the Mendocino Coast of California and responsible for the protection of large segments thereof. His efforts, along with those of his wife Jean Jenny, likewise saved from utter destruction the unique laterite soil near Ione in Amador County and its complement of endemic plants. Not only has he worked to preserve these areas for posterity, he continued to study them, to try to understand why they are unique and why they are there. Today, in May of 1989, you can find Hans in his Oxford Tract greenhouse/laboratory, seeking additional answers to the origin and dynamics of the Pygmy Forest.

Hans Jenny wrote still another book, one that is little known outside of California: E. W. Hilgard and the Birth of Modern Soil Science. Published in 1961, mostly at his own expense, Hans found that publishers did not fully appreciate the importance of building up the image of the first director of the first agricultural experiment station in the United States. I think that this book is as much a monument to Hilgard as is the soils building on the Berkeley campus or the beautiful street on the North side, both of which bear Hilgard's name. The rural life that Hilgard knew has not been preserved, in spite of the lofty inscription on the wall of Hilgard Hall. Citified students think of soil as dirt. Agriculture has gone the way of big business. Many of California's distinctive soil types and series have been covered with concrete and asphalt. Even the soils department

itself has experienced a deemphasis of the importance of its discipline. Hans Jenny's many contributions to the field have helped to keep Soil Science alive. His students are scattered all over the world. People in Nairobi, Haifa, New Delhi, Thessaloniki, Prague, and England ask me about Dr. Jenny. Just a few years ago some Tanzanians may have seen him going up the slopes of Mount Kilimanjaro, collecting soil samples along the way. In England the BBC's David Bellamy produced a documentary entitled Bellamy's New World, a segment of which (The Declaration of Dependence) highlights the Pygmy Forest, emphasizing the close dependence that soil and vegetation have on the state factors of ecosystem formation, popularly known as "clorpt," an acronym known to soil scientists, ecologists, and students in many countries.

Soil Science has no better friend than Hans Jenny; soil itself has no better friend, and the feeling is mutual. Everyone should read the delightful essay, My Friend, the Soil: A Conversation with Hans Jenny. To Hans, soil is much more than an aggregate of anions and cations mixed with a bunch of tiny living organisms. Soil is the seat of health in ecosystems. Soils are species like plants or animals that may become threatened, endangered, or extinct if not thought to be important in our culture and therefore preserved. The appreciation of soil does not 'belong' to science alone or to pragmatic agriculture; it is also part of art (a notion beautifully illustrated in Hans' classic Vatican paper). Soil is the basic component of landscapes, and can be arranged as colorfully layered profiles, as wavy catenas, or as time-sculptured staircases. I have heard of few Renaissance men who were capable of viewing their subjects in so many different ways.

Finally, I'd like to recount some memories. What is it that I have enjoyed most in my association with Hans Jenny? What will I remember best when I am ninety? Certainly I cherish the casual fireside chats at Melburne, in the Jenny farmhouse near Comptche, only eight miles from the Pygmy Forest. Evenings, after a day of field work and a home-cooked dinner, Hans would start a fire in the fireplace and I would recline in the old Pullman chair. Relaxed conversation begins, far ranging and without time limit. Eventually today's data are taken from Hans' battered brief case and discussed in the context of what was found before. Clever thought experiments are tentatively designed to answer a crucial question. Thought experiments, with pencil and paper, often preclude the need for getting volumes of data in the field. This is an efficient way to do research. The Pygmy Forest and its podsol soil is very sensitive and destructive research techniques must be minimized whenever possible. This is where I learned how to study ecosystems. Hans Jenny showed me how to have fun in doing research, how to strive for powerful theories, and how to insure that whole ecosystems remain for others to enjoy and study. With Hans as example, being half reductionist and half holist seems to be a pretty good combination.

Educators, soil scientists, and ecologists will remember H. Jenny by his books, papers, and brilliant research. Those who have had the privilege to know him personally, will remember him, as I do, as a most human person: easy to talk with, tolerant of any and all ideas, patient with everyone, and completely unpretentious considering his stature as one who has been awarded the Berkeley Citation.

Arnold Max Schultz
Professor of Forestry and Conservation

8 May 1989
Berkeley, California

INTRODUCTION BY ALLISON WARNER

I first met Hans Jenny when I began working for Arnold Shultz as a research assistant, following my graduation as a bachelors student from the UC Berkeley Department of Plant and Soil Biology, in September, 1986. I was, of course, quite in awe of Professor Jenny as one of the founders of modern soil science. I was impressed also that he was still involved in research and field work at such an advanced age, at that time, eighty-seven. These two factors made me quite nervous in working with him at first.

Hans, as I came to know him, soon dispelled any insecurities I had in working with him. He has a wonderful ability as a teacher to inspire confidence in his students. I soon, in fact, found Hans to be one of my favorite people to talk to. He has such an inquisitive and open mind. He will freely discuss any topic, from the theoretics of thermodynamics, to psychology or Marxism. I found that he was especially interested in what younger people think, how they perceive the world, and their views of the future.

One of the things that has impressed me about Hans is that he is such a progressive thinker. Rather than having been molded by one of the many eras he has lived through, he seems to have drawn what lessons he could from a particular period, and then moved on to openly meet whatever the future might bring. I will never forget the time Hans asked me if I considered myself a feminist. I replied that I wasn't sure, because while I never felt I had held myself back as a woman, I also did not consider men, strictly speaking, as the enemy,. "But," Hans replied dryly, "ultimately they are, aren't they?" This, from a man born in the nineteenth century!

Working with Hans on greenhouse experiments at the Oxford Tract, I feel I have had the opportunity to witness what has made him such a great scientist, besides his open and searching mind. He is a meticulous worker, making careful observations during the course of his experiments. If there is one thing I have gained from Hans, it is from watching him design experiments with extreme care, to avoid possible errors. I have often been delighted to find that he has assumed errors were going to happen, and our work has been carried out so that when they did occur, we were able to minimize them.

As has been noted to me by one of his former students and collegues, Albert Ulrich, Hans is a master at interpreting results and displaying them graphically. I have found that although he has an open mind, he also is a very critical thinker. It has been quite instructive to me to see how he has come back with the data we have collected. He is always searching for the theorectical framework and larger implications of his and Arnold Shultz's work. Many evenings, after a long day of field work in Mendocino, I would be exhausted; however Hans would press on discussion of a particular point that concerned him, often until quite late.

Hans is very concerned with the applicability of any theory he has to the "real" world of nature out in the field. Likewise he is wary of results and experiments from other workers that are carried out solely in the laboratory. I have been amazed to find that in addition to his many talents as a soil theorectician, chemist, mathematician, teacher, and artist, Hans also prides himself on being adept with his hands and in fieldwork. At first I was

terribly afraid something would happen to him while we were working in the field; but after having spent many full days with him in the Mendocino Pygmy Forest, both rain or shine, I can testify to his strength and endurance. His ability to remain active, in field work, at the age of ninety, rests on tremendous mental strength and motivation that have been an inspiration to me.

It has been an incredible gift to me to have worked so closely with Hans. I have come to know and love Hans, not only as a superb scientist, but as a deeply sensitive person, considerate of others. As his oral history will show, Hans's life has spanned an incredible course of change in human society. I only hope that I can adapt so well to the changes facing us in the future.

Allison Warner
Research Assistant
Department of Forestry
and Resource Management

26 May 1989
Berkeley, California

HISTORY OF THE HANS JENNY ORAL HISTORY PROJECT

The Interview

This oral history with Hans Jenny has its own history, one that stretches back more than a decade. It began in 1978 when soils science graduate student Douglas Maher came into the Regional Oral History Office and suggested that Hans Jenny be the subject of an oral history. Willa Baum, head of the office, readily agreed that Hans Jenny's long and distinguished career as a soils scientist, teacher and author, linked to his phenomenal memory and facility for clear and succinct expression, made him an ideal interviewee.

She asked Professor Paul Gersper, then chairman of Conservation and Natural Resources Studies, to secure funding and provide guidance for the project. Gersper's efforts resulted in two initial gifts from Dr. Harry Wellman and Professor Albert Ulrich. Because of the enormous admiration Professor Jenny inspired both within the university and California agriculture, a challenge offer of $5000 from an anonymous donor led to a grant from University Extension through Harold Heady. The project was launched.

Professor Jenny had published extensively. Doug Maher writes in his interview history which follows, that he and Jenny spent many hours, individually and in collaboration, considering what avenues to pursue in the interviews so that the essential elements of Jenny's life and career would be documented without duplicating information already available. Through Maher's probing questions and Jenny's cogent answers, the reader will learn why Jenny took certain career paths which affected his research and writing and inspired his interest in art and land preservation.

The approximately twenty-four interview hours were begun in 1978 and completed in early 1983. Months, even years elapsed between interview sessions, during which periods Maher and Jenny were traveling (Jenny climbed Mt. Kilimanjaro in 1981 to collect soil samples) or were too busy to take time for oral history.

The tapes were transcribed in the Regional Oral History Office, after which Maher and Jenny reviewed and edited the transcriptions. They deleted some repetitious sections or rearranged others to preserve the chronology or the thread of a particular line of inquiry. Professor Jenny often added information or clarified a point, writing in script in his characteristically spare prose style. At that point, Doug Maher left to carry on his work in Botswana.

In September 1983, about six months after Maher's final interview, Kevin Stuart, a graduate student in soils science at the University of Hawaii, appeared in the Regional Oral History Office to tell Mrs. Baum that

he was preparing to interview Hans Jenny for his Ph.D. dissertation. Because his approach would be different from Maher's, Mrs. Baum asked him to record his interviews and donate the tapes and transcripts to The Bancroft Library as part of the Hans Jenny project. He and Jenny agreed.

During one month Stuart interviewed Professor Jenny approximately ten times, recording about seven hours. By December 1983 he had transcribed his notes and tapes, and he and Jenny had reviewed and edited the transcriptions. Then Stuart left for Honolulu to carry on his work as a doctoral intern in the East/West Center.

Kevin Stuart in his interview history discusses his working relationship with Professor Jenny. There is some overlap in the two interviews but each is handled so differently that the redundancy adds to the interest and the value of both memoirs.

With both interviewers gone, Professor Jenny heavily engaged in his own research, and the funding depleted, the project was temporarily put aside.

The Editing

In 1984 a tentative start toward completion of the project was begun by ROHO editor Ora Huth, but lack of time, lack of funds, and Huth's retirement in 1987 stopped progress again. In August 1988 Mrs. Baum assigned completion of the project to me with the stipulation that it be completed by or soon after Professor Jenny's ninetieth birthday in February 1989. Although much had been done by Mrs. Huth, for which I am grateful, considerable work remained to be done.

For me, a social scientist, the terminology of soil science and its related fields kept me referring to the dictionary. My most valuable assistance always came from Hans Jenny. He took time to check spelling, to review and edit, to proofread. He suggested Professor Arnold Schultz and graduate assistant Allison Warner as introducers, and he provided the illustrations I requested. It has been a singular pleasure to know and work with him. I can well understand what prompted Maher and Stuart to want to interview him and why they produced such fine interviews.

The substance of this oral history belongs to Jenny, Maher, and Stuart. It was, however, my decision to provide a table of contents with many subheadings and to confine the index primarily to proper names. The two are complementary.

I do regret, as I am sure others will, that time and funding constraints prevented us from expanding the oral history to include Dr. Jenny's post-retirement activities: his climb of Mt. Kilimanjaro, his efforts, with those of his wife Jean to preserve the natural areas in Mt. Shasta, the Pgymy Forest, Apricum Hill, and Jepson Prairie, as well as his many special interests listed on page two of his vita. At the end of the

interviews is his short essay covering his concern with soil preservation. Much more could have been gleaned through questions and answers. Perhaps some day others will add volume II to the Jenny oral history.

We take pleasure in acknowledging those members of the University of California's scientific community who contributed funds and personal effort toward completion of this project. Without their generous assistance this excellent oral history might still be in a drawer: mentioned earlier Professors Paul Gersper, Harold Heady, Harry Wellman and Albert Ulrich, who helped launch the project. More recent contributions came from James Kendrick and Kenneth Farrell, vice-presidents, the Division of Agricultural and Natural Resources, Berkeley; Lawrence Waldron, Chairman, Department of Plant and Soil Biology, Berkeley; Lanny J. Lund, Chairman, Soil and Environmental Sciences, Riverside; Kenneth Tanji, Director, Kearney Foundation of Soils Science, Davis; Henry J. Vaux, Jr., Director, Water Resources Center, Riverside; and Professor Michael Singer, in the Department of Land, Air and Water Resources, Davis. From outside the university, the Soil Science Society of America, through its President John J. Mortvedt, sent a contribution to insure the completion of the oral history of Hans Jenny, "one of our most illustrious members."

Malca Chall
Senior Editor

18 April 1989
Regional Oral History Office
486 The Bancroft Library
University of California at Berkeley

INTERVIEW HISTORY BY DOUGLAS MAHER

Malca Chall
Regional Oral History Office
The Bancroft Library
University of California
Berkeley CA
USA 94720

13 January 1989

Dear Mrs. Chall,

Thank you for your letter of 11 November, 1988. Unfortunately it reached me just amidst the chaos of departing on leave; now I am amidst the chaos of returning and getting back in sync with our project. The result is, probably, that this material is too late. But I will try now, and as there is still little time, I will just write off the top of my head.

Let me start with a brief biography so that why I came to interview Hans Jenny falls into context.

I have always been interested in history and natural history. I studied history at UC Berkeley (grad. 1966), completing a senior historiography paper on the post-war history of community organization in North Richmond, CA, with much field research, including extensive interviewing. From there I tried my own hand at community organization in a small village in Botswana. From that experience, I decided to work in rural development, paying special attention to the effect of land use on the value of the land as a resource. Because the soil is a foundation for land use, I entered graduate school at UC Berkeley to study for a Masters Degree in Soil Science.

There I encountered Hans Jenny. I just saw him crossing campus one day. I didn't know who he was, but I liked the way he looked and decided to get to know him. It turned out that he was a soils professor emeritus, and studied soil from the vantage of soil as a naturally occurring body. I not only liked him, but found him an invaluable resource to me in my interests. As I got to know him better I noticed that he had a phenomenal memory. As I got to know soil science better, I realized that he had not only witnessed many great periods of growth in this science, but had contributed very fundamentally to that study. Putting this all together, I realized that this person's oral history should be recorded, and so suggested to ROHO. They in turn invited me to do the interviewing. Of course, as a grad student with a family, I barely had time for it, and that accounted for the interviews being spread over such a long period.

He was willing, but remained constantly skeptical of the utility of the undertaking, partly out of modesty, but mostly because of the inherent unknowns of an oral history, which is meant as primary historical material

for historians unborn, whose queries remain unknown. When once I pointed out vehemently that there is no telling what the historian 50 or 100 years from now will consider of value, he looked at me with his calm wry humor and commented, "Oh, I didn't know you were such an optimist."

As in everything he undertakes, however, he was thorough. The difference between his competence and abilities and my own was ludicrous, but I didn't let that bother me; rather I plowed ahead with fascination. We developed a work pattern before starting, and stuck to it pretty well. First we discussed what should be covered in fifteen interviews. Then for each interview, we would discuss the material in more detail. Then he would write, by hand, what he could recollect and thought pertinent. Then I would study that material in tandem with whatever other historical materials I had time to study. Finally came the interview.

There was a great paradox involved in this approach, however. If he had already produced a written document, then therein already existed that desired primary source for that material, and the interview would be redundant to the historian, to say nothing of boring to us! So we agreed, with some amount of apprehension and doubt by him, I believe, that the point was for me to probe and question in such a way that he would discover insights which were new even to him. This was quite a tall order for a dwarf like myself matched with a giant like himself, but we were of such different temperaments that there was some success.

I did venture a guess at one thing future historians would consider interesting: that was why he should choose to attempt such a comprehensive, integral view of the soil as a whole body manifesting the collusion of numerous "factors of state," as he puts it, amidst the proliferation of specialists in the sciences. So I was constantly probing anything which might shed light on his personal proclivities in this direction. He always teased me about searching psychological depths, but I never probed far anyway because he was explicit that he drew the line between the professional and personal, which was not fair ground under our cooperative agreement. But I didn't really probe much in a directed way, but more out of spontaneous and native curiosity.

Hans Jenny entered academia when it was still plausible to be something of a renaissance scientist, and he lauded his mentors who held rigorous scientific sway over broad domains. But he boldly and competently entered the specialist's domain of colloid chemistry as well as that more general domain of factors of state, and even to that domain he attempted to bring the intellectual rigor, if not ultimate quantitative results, of a mathematical conceptual approach.

Throughout his life he has remained persistent in entering new areas and keeping abreast, e.g. in new instrumentation, and in statistics and computer modeling. But at the same time he has disdained research being led "by the nose" as it were, by each new generation of techniques, rather than more deliberately harnessing new techniques with a purpose derived from an overview of what directions are the most valuable to science. He has

maintained that a moratorium could be called on new research in soil science for a good decade while the mountain of existing papers was more properly digested.

All the same, I believe that he chafed somewhat defensively at lack of parity within the academic ranks for soil science as a discipline in and of itself, corresponding to the view of soils as whole and unique bodies. He often wondered aloud what it would take to put soil science "on the map," and I feel that this sentiment underlay much of his skepticism about the utility of the oral history interviews.

The interviewing began in his office on campus, but we soon shifted to his home. As typing became available at ROHO, I began proofreading the texts. Then I handed them to him for proofreading, especially for spellings of proper names and for clarity of expression during the interview. Finally, I began editing the texts, cutting and pasting, combining his written revisions with the typed interview. At times during interview, I felt it wise to return to earlier subjects, for another go at it, and this involved developing the context again, warming up, and this led to repetition which frustrated him terribly, as he enjoys a great economy of style. More severe intervention by an editor would remove the repetition but at the same time the context of such value to the careful historian. Of course, the most serious historian would presumably listen to the original tapes.

We only completed twelve of the fifteen projected interviews, owing to the necessities of my work carrying me abroad and keeping me too busy establishing myself in my work. However, we discussed this at my request in 1985; we felt that primary sources were available to the historian either in Kevin Stuart's interview, or in other materials, covering the topics we envisioned. I remain with weak conviction of this, and regret the lack of follow-through.

You note that we obviously had great respect for one another: it is easy to understand why I respect him, but more significant that he respects me: he is one who respects anyone who comes honestly to dialogue with the spirit of enquiry.

Douglas Maher
Interviewer
Maun, Botswana, 1989

cc: Hans Jenny

INTERVIEW HISTORY BY KEVIN STUART

b. 1955
BS Forest Management, Oklahoma State University, 1977
MS Forest Soils, University of Missouri, 1979
Ph.D. in progress, American Studies, University of Hawaii-Manoa
Dissertation Title: "The Sad Earth: Social and Ecological Consequences of the Application of the Conventional American Soil Science Model"
Current Employment [December 1983] Joint Doctoral Intern, East-West Center, Honolulu, Hawaii

In collecting information for my dissertation, I interviewed professors of soil science at the University of Kentucky, Cornell University, University of Wisconsin at Madison, The Rodale Organic Research Farm, Alabama A&M University, and the University of California at Berkeley. This information, in conjunction with various historical documents, will be used in the dissertation to describe the discipline of soil science in this country in terms of the evolution of the discipline, structural composition (theory, practice, and research), and the ideas that soil scientists had and have about soil and their work.

Dr. Jenny was interviewed because of his important basic contributions to the discipline of soil science, his participation as an officer in the Soil Science Society of America, and his acquaintance with figures important to the development of soil science. While at Berkeley I interviewed Hans Jenny ten times. The tapes, notes, and transcriptions made from those interviews are on file in the Regional Oral History Office. Both Professor Jenny and I edited the transcriptions. All interviews took place in Dr. Jenny's campus office.

Kevin Stuart
Doctoral Intern
East/West Center
Honolulu, Hawaii

16 December 1983

BIOGRAPHICAL INFORMATION

(Please write clearly. Use black ink.)

Your full name Hans Jenny

Date of birth Feb. 7, 1899 Birthplace Basel, Switzerland

Father's full name Johann Jenny

Occupation Business Birthplace Basel

Mother's full name Marie Th. Althaus

Occupation Housewife Birthplace Meiringen, Switze

Your spouse Jean K. Jenny

Your children Joanna Jenny, Ted Jenny

Where did you grow up? Basel, Meiringen, Switzerland

Present community

Education Public schools, farming, Federal Techn. Institute (E.T.H.), Zurich, Doctor's degree

Occupation(s) Professor Emeritus

Areas of expertise Soil Science, agricultural and colloid chemistry

Other interests or activities Conservation, art, plays, science, tropical landscapes,

Organizations in which you are active many

DOUGLAS MAHER INTERVIEWS HANS JENNY

I THE EARLY YEARS: FAMILY LIFE AND EDUCATION, 1899 TO 1918

Early Years in Switzerland: Basel and Meiringen##

Jenny: I will begin with my early life in Basel, (Bâle, Basilea, Basle), in northwestern Switzerland, bordering Germany and France. Basel was founded by the Romans. I remember that as kids we played games in the ruins of the ancient Roman amphitheater. I was born in Basel on February 7, 1899.

Family Background and Early Schooling

Jenny: My great grandfather Jenny-Wunderlin moved from the country (Gelterkinden) to the city of Basel and worked in the silk dyeing industry. One of the sons, my grandfather Johann, had a chimney sweep business, but he died early. My father Johann was an only child and was a business accountant. As a hobby he played the violin. Mother was from the Bernese Oberland and played the zither. My own musical accomplishments remained moderate. I was a drummer in a marching band and later played the guitar and sang with my sister (Johanna or Hanni) and friends.

I went to grammar school in Basel in a boys school, with classes held from eight to twelve and two to five. At 10 a.m. was a coffee break when poor kids got free, hot soup. I brought along bread and chocolate from home. At 3 p.m. fresh goat's milk was available from a goat herd that came to the adjacent park. We had male teachers only who applied corporal punishment for minor offenses.

##This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 254.

Meiringen: Introduction to Agriculture and Art

Jenny: For about three years, between 1908 and 1912, our entire family, parents, Hanni and Hans, moved to the town of Meiringen in the Bernese Alps to live with Mother's parents; my grandfather was Peter Althaus-Amsler. They owned a wood-carving factory and needed father's business knowledge. It was a wonderful period for me.

Grandfather and my two uncles were artists. I absorbed words and ideas from their often animated discussions on art. I admired their creations in the then "modern art" form, which is now called Jugendstil or youth style. I have been hooked on art ever since.

In Meiringen, I was introduced to agriculture, spending much time on the farm that was adjacent to our big house, which still stands. Decades later I used to tell agricultural audiences that my first job was as a fertilizer salesman. In Meiringen we kids mounted wooden boxes on small wheels, collected horse manure on the streets and sold it for a few pennies.

The schools were coeducational, which was a great novelty to me. We were the first skiers in this mountain valley (Haslital), making our own skies from staves of sauerkraut barrels. Skiing remained my favored sport.

My grandparents were important people in the town and valley and I felt that in town I was treated less casually than in Basel. Somehow I had an identity that people seemed to respect. I was "Hans Jenny von Basel."

Return to Basel

Jenny: Upon our return to Basel my parents had to decide what type of junior high school I should enter, one that prepares for business and the trades, or one that eventually leads to college. Mother insisted on the latter. Rigorous training began that transformed a naive adolescent into a sophisticated, somewhat smart-alecky young intellectual. Throughout this period World War I raged at the Swiss frontiers.

School subjects centered on languages, history, mathematics, and sciences. Classes lasted all day and home work kept us busy until after midnight. Since Basel had a Protestant state church, in these pre-high school years the first hour each day, 8 to 9 a.m., was devoted to religion. I was receptive and fancied myself some day becoming a missionary preacher in India where the Basel Mission Society had an establishment and school.

The High School Years: 1914 to 1918

[Interview 1: 1978]##

Jenny: I would say that the period from 1914 to 1918 through the all-boys high school, was a rather traumatic experience because the demands were intense. Incidentally, there were two kinds of high schools that prepared you for university work. One stressed the humanities, especially Latin and Greek, and the one that I was in stressed modern languages, history, science, and mathematics. If you pass through one, it is very difficult later to switch professional pathways. The one that stresses the classics leads you into the ministry or philosophy, the humanities and medicine, whereas my training was more the teaching of modern languages, engineering and the more scientific-technical aspects of life. I remember when I was in college, we had a class in the zoology lab. We had to dissect a frog and the teacher called all the students together around me and he said, "Look at the man's hand. He's a born surgeon." This gave me an idea that maybe I should explore that. But it turned out that it was a completely hopeless case because to enter medical school required eight years of Latin and four years of Greek, studies requiring eight hours a week, and I didn't have it. The medical schools didn't have any quotas, but a quota was set up by these requirements that let in only people who had parents already in these fields or who knew about the humanistic intellectual group. I believe that these severe classic requirements are no longer in force.

Maher: How did you feel about that when you discovered it?

Jenny: It wasn't an upsetting thing. Then I was already aware that many of the courses that we had to take and the diplomas we needed were all hurdles that were probably economically conditioned. It hadn't so much to do with the subject matter. I knew you didn't need eight years of Latin to be a good M.D.

Early Social Awareness and Exposure to New Ideas

Maher: When do you think that you became aware of that? How did those ideas form within you?

Jenny: The ideas? I would say I became aware of the structure of the social environment during the war. At night we heard the thunder of the cannons--we were only a few miles from the battlefields--or high up we could see planes escaping into Basel territory pursued by puffs of smoke from anti-aircraft artillery. Also, thousands of

Jenny: German and French wounded soldiers could come into Switzerland and were treated there. Allied and German war propaganda was at a continual high pitch.

So I remember that when we were in class discussing the poetry of Goethe, and then if we looked out of the window we saw all of these crippled soldiers standing around. The whole thing didn't add up, and fairly soon, all the propaganda and the claims that God was on the left or the right side, made us very cynical. We concluded that, "They are just fighting for colonies and their material possessions, and millions of people are being killed for these things." I wouldn't call it revolutionary because it wasn't, but we were very skeptical about the teachings in our educational system.

Maher: Were you by now around fifteen years old?

Jenny: I would say either sixteen or seventeen. Here in the U.S. you have no idea how the war period was. There were many French people and a lot of Germans living in Basel and they lost their sons, and if you knew them, it was an emotional experience. So the study of grammar in minute detail and memorizing all of that, especially the readings in literature was just too much of a contrast to be taken too seriously.

But, strangely enough, after the war these intense impressions faded out. New things came and--[pause] I also had difficulties in high school. Now I feel that at that time we had to memorize too many details and in some ways we were overeducated, but in other areas we never learned a thing. We never heard anything about psychology, nothing about sociology, nothing about evolution. These important fields were not taught. I felt we always lived in the Middle Ages, or in Greek and Roman times and never got near to 1800, never got to the present time in history and languages. It was always the old things.

Now, in high school we circulated what you might say was "subversive" readings. That is not the right term because it wasn't really considered subversive. For example, we read intensely and even discussed a book by Henry George in class. The name was perhaps Progress and Poverty. We even wrote simple equations about parts of it. I remember that we put the poverty numbers in the denominator or in the numerator as we played around with the equations. I read Prince Petr Kropotkin (translation) and much of Voltaire, especially Candide.

Some of my classmates even claimed they read the Communist Manifesto. I don't know if that was true. So all these ideas and questions were really boiling and, of course, after the war there was a great threat of Communist takeovers, even in Switzerland. Yet most of us were patriotic. I served in pre-army training units, became a sharp shooter and spent a week in Swiss frontier trenches.

Jenny: Although I have many negative impressions about the high school years--it meant classics all day long, and then at night we had homework until midnight. We also had many positive experiences. We certainly became aware of the cultural background of our city, of Basel, and we became familiar with and identified with some of its great people like the mathematician Leonard Euler. We practically lived with him. We used his algebra textbook, which was written two hundred years earlier. It was such a beautiful thing. Or the Bernoulli brothers (or cousins), who were early calculus wizards, or a fellow we learned a lot from, a Swiss historian, Jacob Burckhardt. He wrote a famous art book on Italy, The Cicerone (1855), a guide to Italian art, and we always carried that with us in our knapsacks when we went vacationing in Italy. We knew a lot about the famous Basel Art Museum.

We had a very progressive art teacher, Niederer. He took us to the latest art exhibits in town and very early we became familiar with men like [Francois] August Rodin, Paul Cezanne, Wassily Kandinsky, Max Beckmann and Pablo Picasso. I remember those pictures. We were in the forefront of what was going on. We studied the architecture of the city. These were very positive gains.

I had some handicaps. As I said earlier, I had a hard time memorizing words and we had an enormous amount of memorizing to do in poetry, history and languages. I had intense extracurricular activities and at time we had a problematic family life. So, summing that all up, I was just an average student and I know I had my share of feelings of frustration, and of being mediocre, which was considered worse than being a criminal. The worst thing you could do was to be average, and my performance was average.

Maher: Did you find yourself adjusting to that idea or constantly beating against it to try to get above the average?

Jenny: No, it had with it a lot of disadvantages. Later on, at the moment I got to the university, I turned out to be an A student. But that's a later story.

Personal Development: The Boy Scouts and High School Experiences

Jenny: During the time I was in high school I was average but the redeeming feature was my involvement with the Boy Scouts. That was just a new thing that had come up. I joined that group and this gave me a lot of self-assurance. I became a scout troop leader and eventually a scout master. I was very effective in the scout movement. So that

Jenny: balanced the inferiority feelinga I had from the rigid schooling, but, of course, it took time away from the school work I should have done.

With the scouts we had field trips going outdoors every weekend and every year we had hikes that lasted two or three weeks all over the Alps. Our troop was rather an ascetic group. There was no smoking, no drinking, no displaying of merit badges, and some of us indulged in such things as fasting for three days. I don't know why we did that, but anyhow this was to me an important phase in my life.

Maher: Had you discussed this idea of not displaying the merit badges amongst yourselves or did you inherit that as the tradition of the troop?

Jenny: No, our group was under the leadership of a school teacher named Lanz who was a fairly religious man. In Europe you find a lot of people whom we would call idealists about life here. They get involved in how a society should be run and all that. The fact that Lanz did join and organize the scout movement in Basel meant that he was an educator who wanted to do something for the young kids. But other scout troops were different. Ours had the reputation for being the more--I used the word "ascetic."

We had one member, a close friend of mine who was an outstanding spokesman, but even we labeled him "the rider of principles." Of course, he was from the high school which stressed humanities, and he talked about Plato and knew all the good things. That was his strong point, and he did that all of his life. He was here in Berkeley not long ago and he hadn't changed a bit.

Maher: Who is that?

Jenny: He is Professor Albert Streckeisen, a professor of geology in Berne. He was a relative of that famous historian, Jacob Burckhardt. Family traditions were very important for people. I saw that in Meiringen. My people were important people, so I was important. In Basel, my people were not important people, so I wasn't important. But Streckeisen, that scout friend--still an excellent friend--came from important people. So he became an intellectual leader by having that background.

Maher: But you became a leader as well through the scouting.

enny: Yes, but not so much in terms of "riding the principles." I was good in nature and getting around and in teaching kids how to cook and make a sleeping bag and a lot of practical things. He couldn't do that. And I would go mountain climbing with the troop and had them on ropes and picks. He couldn't do that; he was scared. So we sort of complemented each other.

Maher: Do you think that this environment of not stressing the merit badges and everything like that was a kind of relief for you after having always been labeled by grading and being considered mediocre in school?

Jenny: No, because I was doing excellently with this merit badge stuff. I knew I was excellent with the compass and trigonometry and all these things. In my reading, and all that--I was just tops.

Maher: But that came forth from you rather than being demanded of you?

Jenny: It came from me and had to do with the agricultural experiences I had. Well, let me tell you how the school was. To the German Swiss, German is a foreign language, yet it is their medium for written expression. For example, we had German composition in high school and we had to turn in a theme every week, two or three pages. Now, the teacher would read this and correct it and then he would discuss each paper in front of the class, sort of making fun of them. The whole class roared and laughed when you used a corny expression or didn't know punctuation and all that. It was very humiliating but it was effective. You learned fast. In fact, we learned composition so rigorously, that fifty years later I could still write an obituary in German for a professor in soils and it was published by a German university--no corrections made. So it was really something, but you paid a price.

Also, our critical faculties were sharpened to an extreme extent. In fact, often we carried the critique methods over to our lives outside of school. I know that my family often revolted against my analytical attitude.

Maher: Can you think of an example?

Jenny: I would say it was mostly at table discussions where, during the war, many statements were based on emotional judgments and, of course, that's where I came in and started to analyze it, to prove or disprove it, or anything like that. This just made it impossible, but that's the way we were trained, to be critical.

Another aspect during the high school years was that as a consequence of the war, food shortages became prominent. Many farmers had to serve in the trenches at the frontier and so appeals were made to high school students to work on farms. I always spent summers on the same farm in the Emmenthal Valley (Gysenstein) and I became pretty good at it. Eventually I was put in charge of the horses and oxen and plowing and had a lot of responsibility.

So that fed back into the Boy Scout movement and I was considered to be much better than everybody else when it came to using ropes or looking at nature or anything. I had a knowledge which the others didn't have.

A Year of Farming

Jenny: Finally high school ended in 1918. Over there, if you pass, you end with a certificate that permits you to enter any university or higher school of learning in Switzerland and in Germany as well; however, not in France. So I decided to study agriculture but before going to the College of Agriculture, I took off for a year and worked on farms mainly in French Switzerland, and also in German Switzerland.

Maher: So to you, did your introduction to soil have that typical agricultural base?

Jenny: I had an agricultural base and I remember one thing that I will never forget. That farm where I worked was in the hilly lower Alps, the "Before-the-Alps," and when we plowed, we always plowed on the contour, which meant the furrow slices had to be turned downhill. You start at the bottom and the first thing done after the first furrow is cut, is that all of the kids in the family came with baskets and carry that furrow up on top of the field. So this was an elementary conservation technique which evolved through the centuries, but I assume that during those centuries the land was actually rotated. I feel that that experience unconsciously influenced my thinking about soils later on. At that time my goals were uncertain but I thought of becoming a livestock farm manager.

Hans Jenny at work on the farm.
Gysenstein, Switzerland.
About age 17, 1916.

Hans Jenny at work in the Pygmy Forest.
Mendocino, California. About age 86. 1985.

II THE UNIVERSITY YEARS: DEVELOPING GOALS AND A LIFE PHILOSOPHY, 1919 TO 1923

Entering Zurich's Swiss Institute of Technology in 1919

Jenny: In 1919, when I got to Zurich where the Swiss Institute of Technology is (it contains the College of Agriculture--it corresponds to Cal Tech except it has the College of Agriculture in it) there was a completely new orientation. I was so eager to learn and understand that within the first half-year or year I was the top student and, of course, my parents were surprised and pleased and my relatives, too.

When I entered the College of Agriculture--in 1919, after a year of farming, in the cantons of Berne and Geneva--my main interests were leaning toward animal sciences. My knowledge of soils was entirely practical; digging ditches and plowing and harrowing with horses and oxen. I had experienced the fickleness of sandy soils, saw that clay soils required heavy Belgian plows to turn a furrow, and that stones interfered with all soil manipulations. On steep slopes, some Swiss farmers plowed on contours.

Textbooks in agronomy and farm management courses dealt mainly with cultivation and drainage problems. On the weekly agronomic field trips, wherever we looked at sites, the soil properties were tied to the surface geology; rocky soils on moraines, sands in river beds and the abundant peats in shallow depressions. This outlook was typical of West European soil science.

What was interesting was that my classmates in Basel--most of them stayed in Basel--had a sort of unwritten behavior pattern that in the first semester you don't work hard, you loaf, and you flunk the exams. So all of my former classmates, at least the ones I knew about flunked their exams the first year. The saying was "You just have to wait for word from Hans Jenny in Zurich that he has flunked, too," but it wasn't so. That changed my relationship with my old friends in Basel.

Maher: How did it change them?

Jenny: Well, I simply appeared to be in a different class or to be a different type of person. I was eager to do things, not just follow the pattern as they would do, to become a teacher, or maybe go into medicine or any other standard career. Also in Zurich I associated with the less conventional students, simply meaning that I did not join a fraternity. We were unconventional. For example, on one of my visits to Basel my relatives were dismayed when they heard I was walking around in Zurich without a hat and without cuffs. That was considered avant-garde at that time.

Maher: Were you conscious about this choice?

Jenny: Yes, yes, I think so, and it had to do with--

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Jenny: --with the war experience, my identifying with farmers, and perhaps that I wasn't interested in meeting again with a lot of people who came from the traditional families, and the fraternities, who, you might say, were the well-settled business and banking establishment.

Maher: Did you feel that these people weren't dealing with the reality that was going on around you, referring back to the contradictions that you saw between the high school classroom and the bombs outside.

Jenny: Well, you see, many of those people didn't see that. The city of Basel was close to the battlefields, whereas in Zurich and interior Switzerland people never heard any thunder, they didn't see the flack. It was a peaceful life. They were not threatened in anyway, physically or intellectually.

Maher: Did these other students tend to be less conventional also in the kind of positive way of being eager like you to strike out into new areas?

Jenny: They would belong to certain groups, such as the Wandervogel, a hiking group, where boys and girls used to go on long tours together. That was, of course, considered very bad in those days. But looking back, we were essentially conventional. But I don't know now how I picked my friends. I know I had a roommate, Rudolph Geering. He was from Basel. I think he was in one of my high school classes. Yes, and he studied chemistry. So I had a lot of contact with him and his friends. I guess my friends were more of a random selection of people, based on how they dressed, how they walked around, how they behaved. These were the little things which showed that you were not a rigid establishment type. There an establishment type was different from here because society was much more conformist and more conservative.

The Institute's Faculty

Jenny: I became enthused about university work. For example, my undergraduate physics professor was a fellow by the name of Auguste Piccard. He was the first to ascend into great heights in the stratosphere in a balloon and he built the first bathyscaphe to go to great ocean depths. I wasn't sure at that time what he was doing. Now I think he may have also measured cosmic rays. But that was just another thing that he did.

Maher: Were you struck by him as a person?

Jenny: There he was a teacher who would not just outline and follow a textbook, but who would tell what he was doing.

The way the class work was presented told you that these were people who lived with their subject matter. That impressed me. They were all very good teachers. In geology I had a fellow named Schardt. He was the originator of mountain folding theories in the Alps. Let's see, who else impressed me? The first time I met Professor Georg Wiegner, he taught agricultural chemistry which included soil science. He played a big role later on. But before then, before I graduated, there was a politician, a famous agricultural politician who taught farm management. He made an impression on me because he was instrumental in elevating the economic status of the farm population.

Maher: Who was that?

Jenny: Ernst Laur. I had a French Swiss teacher by the name of Jaccard. He taught plant physiology, but he is now world famous for having been the first to devise a mathematical similarity index of plant communities. But that was never mentioned when he was there at the institute. He had given up that field or had to give it up. So I learned to appreciate him after I left.

Maher: What gave you the impression that he had <u>had</u> to give it up?

Jenny: Oh, I know that. He was an ecologist. Then there was a professorship open at this federal technical institute. There had to be a certain number of professors from French Switzerland and Italian Switzerland. So it was decided that this chair in plant physiology would go to a French professor. He was to lecture in French and we had to listen to him in French but he learned enough German to talk fluently. He had to give up his ecological interests and devote his time to plant physiological questions in order to get that position. There is a very complicated scheme as to who gets what position.

Maher: What was the term used at the time for his studies in ecology?

Jenny: Plant sociology.

Maher: Was the word ecology used at all?

Jenny: No, the expression "ecology" was coined by a zoologist in Germany, Ernst Haeckel. I heard negative remarks from people about the ecologists in the United States. They said these ecologists really didn't study botany and plant sociology. They just fool around with ecology. But I think these judgments were erroneous and really reflected a certain snobbishness and self-defense. When some people feel threatened, they start attacking, and that's what they did. But it made no impression on me.

Maher: So this is your view in retrospect, whereas at the time, you just heard it.

Jenny: I would say I heard that later when I was well along with my Ph.D. work.

So I took all of these agricultural courses, with a heavy emphasis on physics, chemistry and the sciences. The first two years emphasized science courses.

Looking back I think it was great training. We had entomology and we had a broad perspective of nature. Aside from scientific and technical courses we were required to take electives, preferably in the humanities. I took art courses and resumed my interest in outdoor sketching.

More on the Swiss Institute Years

[Interview 2: no date]##

Maher: What shall we discuss today?

Jenny: The notes I have jotted down deal with student activities, student life in Zurich and general remarks about the teachers. I noticed that as I think about the period, my evaluation of a person changes as I think about them.

Maher: Like Georg Wiegner?

Jenny: Like Wiegner, yes.

Maher: I've noticed that I've been becoming more and more aware that my impressions of your viewpoint are changing.

Jenny: I knew Wiegner when I was a student and then now--years later, looking back on Wiegner--I see him in a different picture. I published an assessment of him in 1936 in Soil Science. So what should be discussed here? The way I saw Wiegner as a student or the way I size him up now?

Maher: I think as long as we are clear as to what period we are looking at at the time it doesn't particularly matter.

Jenny: Well, okay.

Maher: I guess we have to recollect that this is primary source material. It's not like a finished, balanced book or documentation. What we're trying to do is to get down in a discussion the things that normally don't get down on paper. Would you like to glance over the notes that you've made today?

Jenny: Well, I'd like to because I've noted some things that you might think are interesting. You should let me know what you think is valuable because I also have in mind a comparison of student life in Europe with what I saw when I came to the United States. The difference was really enormous, especially when I was at the small colleges, the small land grant colleges, where universities are put into small towns. I'd like to discuss what this means in terms of the development of young people, both positive and negative.

Maher: Would you feel comfortable dealing with that today, speaking about the student life in Zurich?

Jenny: Well, we could take a stab at it.

Maher: Then I have some things I'll be asking as they come to mind. Why don't we just go ahead then on that basis?

Jenny: The trouble is that now what I'm going to say is partly repetitious.

Maher: That's fine.

Jenny: As I explained earlier, I had spent a year on farms. I lived in small towns, isolated little hamlets, and you might say that there was no intellectual life, although it was a rich life with these people. They had sound philosophies about life, no book learning about it, but what it really is. This was a revelation.

Nevertheless, when I went to Zurich in 1919 and enrolled in the College of Agriculture at the Federal Institute of Technology (E.T.H. or "Poly") adjacent to the University of Zurich (called "Uni"), I got immersed into an intense intellectual life because in the postwar period Zurich was a great international center. A lot of people, who were displaced during the war, and new movements flocked to Zurich. Just a few years before I got to Zurich, Nikolai

Jenny: Lenin had left and gone to Russia and now he was the big man over there and the newspapers were full of what he was doing in Russia. The art movement called Dadaism was still around, and some of the survivors met with the students in the cafes (e.g. at the Odeon) and we talked about it. There were people like Albert Schweitzer, an organist and a medical missionary in Africa, who lectured to large audiences. We heard about [Mohandus K.] Gandhi. There were new plays and there was, for example, [Igor] Stravinsky. His music insulted the classically oriented people in Zurich. So everything was highly activated.

Maher: How did it strike you, having just had a year of rather stable experiences, as you depicted it, both in terms of what was going on (because it was isolated and rural) but also because there was a solid philosophy that was intertwined with the living. What did it feel like, coming from that to this tremendous variety and all kinds of different viewpoints being played against one another?

Jenny: Well, I would say it was exciting, just as if a country boy comes to San Francisco and sees all these wonderful things. It's just like a smorgasbord; you eat everything! And probably end up with indigestion.

Maher: Was it an opening up for you in the same way that maybe Meiringen had been for you earlier?

Jenny: Well, it was different. What impressed me, was the core was always the "Poly" and "Uni" with their excellent teachers who, you felt, knew their stuff thoroughly and I knew I could get what I wanted to learn. As I told you, in high school I was an average student which was considered worse than being a criminal. But here, I was so eager to learn and it came so easily, that I became an "A" student right away and that gave me a certain prestige which I needed.

Comparison with the High School Years

Maher: What was the shift here? Was it because the high school years stressed memory and technique or was it more that now you were really amidst the subject matter and the reality of these growing fields of knowledge? What was the difference? This is a bit of a puzzle to me.

Jenny: I think during a year in farming, I became aware of certain technical, scientific things I had to know and that I wanted to know. The information was available and of high caliber, so that was a great satisfaction. I might add that at that time there were

Jenny: no fellowships available and doing part-time work was unheard of, so all my expenses were paid by my father and I was careful not to use too much money. I had complete freedom from financial worries.

There also was the freedom from examinations. We had no midterms and we had no finals. So the evenings were absolutely free and we had all of the discussions and could take in all of the concerts and readings, and lectures. It was great freedom.

Examinations came at the beginning of the succeeding semesters and before that you had several weeks of vacation. During the vacation you prepared for the exams.

Maher: [laughs] That's a misnomer!

Jenny: Yes, but anyhow, during the semester you could absorb freely because you didn't have to worry about having to dish out things. The lectures were stiff and formal, but most were accompanied by either lab periods, field trips, discussion periods or something. So half the time was devoted to actually digesting the lecture material.

Maher: The big difference then was that you had a purpose and you could see a close tie between what you were going to take in, even though it was rigid and abstract and it included practical lab experience. But you could tie it to something concrete that you saw yourself doing. Had there been an absence of that in high school, a time when everybody was learning all of these basic things and didn't relate to anything, especially the war?

Jenny: That's right. In high school good material was presented, but it lacked relevance to what I wanted to do and in high school I didn't know exactly what I wanted to do. Also, being away from home was a good thing. My mother had a lot of illnesses and that was depressing, but being away wasn't such a heavy burden. [pause]

The students I was associating with were not the typical establishment type, and I did not join a fraternity. So, you might say, we were more outgoing. We wouldn't wear hats or cuffs, which my conservative relatives in Basel objected to when they heard about it.

The Students' Diverse Interests and Broad Outlook

Maher: Did these students have a similar outlook to yours in that they knew that they were going places instead of just marking time in an institution?

Jenny: Yes, you see this was the Federal Technical Institute (poly) and most of the people who went there had a more or less definite idea of what they wanted to be, compared to the broader letters and science fields of the university where students are not yet necessarily sure what they are after.

Maher: I'm sure that they all had very divergent interests, but can you see patterns concerning that generation you were with that maybe set them apart from earlier people going to the Technical Institute?

Jenny: Well, I would say the war had its effect, and the people I went to classes with were exceedingly heterogeneous in terms of outlook and experience; you might say, ultra-conservative farmers active in politics, extreme socialists, Marxists, all these notions were in the air. There were also the eastern influences, such as the German Theosophic Association, the anthroposophical society of Rudolf Steiner, and the Mazdasnan people. A private teacher Tschulok, gave public lectures on the philosophy of evolution. But Sigmund Freud and Carl Jung had no public following, as far as I remember. There was a tremendous spectrum of interests and activities, and I took in many of them, but not very profoundly. So actually my range of friends and people I had contact with was very broad. It was not a narrow segment with a certain viewpoint.

Maher: But they all shared this common feature of knowing what they wanted to do?

Jenny: Yes, all of the people I was with, including the people I knew who at that time were in chemistry or biology, were discussing the really broad issues in these fields.

Maher: Was there a group that was fraternity oriented? Even though they were at a technical institute, were they still doing what they were supposed to do or marking time?

Jenny: Yes, Zurich had the fraternities, although, as an after-war casualty, they were not as prominent. No fencing was going on; it was really a social thing with drinking parties. In agriculture we had a club, a fraternity you might say. I was not a regular member; every evening there was a reserved corner in a restaurant where you could go and drink and meet your fellow men. You stood out because you had a colorful cap on, and you had these old boy contacts. But I was not a very active member of that group. I had seen hardships during the farming year out in real life and so I felt no great kinship. In their ease it was simply a continuation of high school activities into the college years.

Maher: That farming year was really pivotal for you.

Jenny: Yes, it was an important year all around.

Maher: Did you know it at the time you were experiencing that year?

Jenny: Well, at that time I was very anxious to learn all of the tricks of the trade, so to speak, such as plowing with oxen. This was very satisfying because I could look back and say, "I couldn't handle a horse and now I can." So this was a plus. Looking back now I really had all of these supports and reinforcements of a positive attitude to life because I started to be able to do things and I felt I would never be in danger. The big danger in Europe has always been famine, lack of food. I didn't have to worry; I could feed myself. I could take land and handle it, so I had a solid foundation which gave me confidence in outlook, and that must have been reflected in the way I behaved, and attacked things, and studied and all that.

Insecurity after World War I, Life Philosophy, and Farm Management Goal

Maher: The postwar period is well known as a period during which people's confidence had been so severely shaken. Afterwards, a feeling of insecurity was rampant, I'm sure.

Jenny: Yes, I'm sure of that. Well, like today in a way. We had all of these religious trends and movements, and oscillations in church attendance. Yes, it was the same way as it is now, with great issues confronting the world.

Maher: It strikes me as well that a lot of people of my generation are wanting to return to the land and gain very concrete skills, and I never thought of it in terms of wanting that kind of security. I've seen it more as a kind of an advocacy about wanting a unity to life.

Jenny: Yes, I think the reason is that in the United States we have never had a food shortage whereas over there, during the war, this was a big threat. It was a real thing to be able to eat and get your three meals a day.

Maher: Now, let's go forward again to the people that you were sharing your university years with. With all of this freedom, this smorgasbord of tasting and so forth, were there any others who had a similar, really concrete rooting in a year of experience out in the real world? Or, were they mostly people who had made a transition directly from high school to the university?

Jenny: I would say that outside of agriculture, most of them just moved from one classroom to another. But in the College of Agriculture, you either had at least a year of agricultural experience (that was a requirement to enter) or you grew up on a farm and acquired a

Jenny: different perspective, unconsciously or unknowingly. No, I think we had a positive group of students and as to the people who were less positive, we didn't have too many points of contacts. I'm sure that the people who studied in the humanities probably had more conceptual problems than we had.

Maher: Before the university years and this very systematic gaining of new skills, did you see yourself becoming a farm manager?

Jenny: I would say that in the first two years in the College of Agriculture this was still my aim, to be a farm manager, especially with livestock. But gradually my interests shifted and, of course, the crucial point was when I got to know Wiegner and started to take his courses in agricultural chemistry, especially those related to soils.

Maher: Before we go to that, can I ask you to relate that view of yourself--as a farm manager being very thoroughly ensconced in the stable pursuit of agriculture? Can you relate that to what has come up again and again as you speak of your early life and the different forms of the missionary view of yourself. First, you literally said that you had wanted to be a missionary. That was when you were quite young, I think. Then you spoke of wanting to be a surgeon which, although it is not the same, it can possibly, from my point of view, fit into a view of yourself as being of service to people. Then you mentioned Albert Schweitzer a couple of times. Did this idea of being on a farm, did it mesh with a larger view of where you were going in the world?

Jenny: Yes, during those years, with the changes in nations--nations appearing and disappearing and upsetting economic systems--there was a lot of talk about socialism, communism, and the types of farm management, plus the idea of changing farming systems to be under either government control or to offer economically sound larger units. So while the College of Agriculture was keyed to the small farmer of Switzerland, I had in mind broader agricultural dimensions that might happen someday that might involve me. I couldn't visualize how it would be, but I thought there was a future in a changing agriculture. But I had no really clear idea how it would come about. Switzerland was a stable country so there was no problem there. But I was looking all over Europe and overseas for what might happen. So that was in the background.

Maher: Did you talk to people about that?

Jenny: I might have talked to some of my friends. Yes, one younger friend who really took on the life which I gave up then. A younger student and friend, his name was Henri Ossent. After he got his Ph.D. in animal husbandry, he went to Africa and became an agricultural advisor to the king of Abyssinia, Haile Selassie and that was all large scale farming. So he did what I once had in mind.

Maher: When you first knew him, you found common ground in discussing this general, unknown future.

Jenny: I would say, not very intensely because he was several years younger and so we did not have very close contact until later years when I was in graduate school and he started graduate school.

Philosophical Thoughts and Discussions

Maher: Did you have a disdain of thinking too far into the future or of getting too philosophical?

Jenny: I was not thinking. I had one childish idea once where I looked into the future. This was after I had some geology in high school and I realized that someday all of these mountains would be flattened and then the Switzerland I knew wouldn't exist anymore and that made me sad. But, of course, that would be millions of years ahead and those thoughts ignored mountain building. But I think that's the only concrete future speculation I remember. Now, I would say that in Zurich I was living from day to day.

Maher: But on the other hand you weren't doing that consciously. You weren't disdaining going off into the abstract system. That's the way it was.

Jenny: No, I was reading. About the second or third year I started the philosopher's club at the home of a professor Sprecher. We met for a year or two once a week, reading especially the works of [Arthur] Schopenhauer to get a perspective of various systems of philosophy and to become acquainted with some of the philosophical problems. We chose Schopenhauer because he was recommended to us by a philosophy professor who said that Schopenhauer would give a broad perspective in discussing many philosophical schemes, especially Kant. These meetings were worthwhile experiences. We made extracts and read points to each other. We would read a chosen chapter at home and then discuss it on a pretty broad basis.

Maher: You met weekly?

Jenny: It was weekly and pretty religiously, yes. These things were possible because we had, as I said, no midterms that would interrupt our leisurely pace.

Maher: How many years did that last at the university?

Jenny: Well, I would say definitely one year, possibly two.

Maher: Beginning the first year?

Jenny: No, I would say the second year.

On Dating Girls and Self Awareness

Jenny: What is interesting from the perspective of present-day activities on a campus is that in those years girls played a very minor role in our lives, at least the first two or three years. First, there were hardly any co-eds (only a handful), and to date girls from the so-called good families you had to have a more definite future ahead of you--as to what you were going to be or what you could offer. So we had few contacts. Later, this changed. Segregation appeared natural, perhaps, because we always had gone to boys' schools. So the absence of girls was not a big thing.

Maher: You didn't notice it at the time?

Jenny: Well, it was not something that was really as important as it became later, after a chemist friend and I initiated a private social dancing course with dancing teachers that was highly successful.

Maher: How did you contend with your own identity as a young man?

Jenny: [pause] I'm not sure how to answer that. I would say that in Zurich I had a pretty good opinion of myself, whereas in high school I didn't. I sympathized with or identified with many characters in literature, especially with some of the young people depicted by Thomas Mann, whom I got to meet later in the United States; also with people in Schnitzler's novels; some figure paintings from the Renaissance period and the famous picture by Thomas Gainsborough, "The Blue Boy," which is in a Pasadena museum. I would identify with him and see myself in him.

Maher: What did you see?

Jenny: Well, a relaxed person with assurance. When I look at old photographs with my friends from those days, they show real happiness and confidence. [goes through notes] Let's see what I have.

Maher: What were some of the novels by Thomas Mann, or works that you identified with?

Jenny: Well, of course, we read Buddenbrooks but also--I've forgotten the name of the one that was about the clever criminal which appeared in a good movie later on. I've forgotten at this moment the names of them. But I identified with those.

Maher: It was actually Thomas Mann?

Jenny: Thomas Mann.

Maher: How about something like Death in Venice?

Jenny: Yes, I've forgotten now what they were.

On Contact with Religious Relatives

Jenny: I mentioned religion earlier. During those years I read a book by a zoologist named Ernst Haeckel on the world puzzles (world questions--Weltratsel) and from then on I became skeptical of religious dogma and was weaned away from it. But this did not prevent me from keeping close contact with one branch of my family, the Brunner family, who, I believe, were Mennonites.

The Mennonites are related to the Amish people, but I did not know that about them at that time. I was intrigued by the lifestyle of these relatives. So when I came to the United States, which was really an eye-opener, I met some of these people and have been intrigued ever since by these relatives who are in a group related to the Amish people, known here under the name of Mennonites.

Maher: Wasn't this in a rural setting and didn't they lead a novel way of life?

Jenny: Yes, but these relatives were not rural people. One was an architect and one was an engineer. They had a close knit society and a community life which we, who belonged to the state church, never had. The state church did not offer emotion and religious satisfaction. That's why you had all of these small denominations developing outside of the state church.

Maher: Is that what attracted you about the Mennonites?

Jenny: Yes, that they had this intense family life and I was always amused slightly by my observations. When they met, there was always a lot of kissing going on, embracing and kissing, and later on I thought that maybe that was one way to resolve sexual tensions embodied in the religion, without these people knowing it. But I got wise to such views later on, not that early.

Maher: You felt yourself attracted to it?

Jenny: Attracted to it, yes, to the way they acted. Of course, later I found in the United States this is really a very common thing among the religious groups here, whereas over there the state church is really a deterrent to that kind of emotional content in religion.

Impact of Literature: Thomas Mann and Hermann Hesse

Maher: It seems to me that in the characters of Thomas Mann, he depicts a balance between or a tension between an emotional side and an intellectual side.

Jenny: [pause] Yes, that's right but also his stories, especially the Buddenbrooks, were really about people that you might say I knew, or myself, about how I was in school and how the other young boys were in school and what we went through. So our daily lives were involved also with the literature and you might say we lived in both the real world and an imaginary world, and I think I still do that. Certainly, when you are on a faculty and in an ivory tower, you don't live in the real world. I found that out after I retired.

Maher: [chuckles] You got back to the real world?

Jenny: Yes.

Maher: You mentioned to me that you did see Hermann Hesse once.

Jenny: Yes, once I attended a public lecture by Hesse. He had a following of people who were older than we were, so somehow we fell between the chairs.

Maher: Could you be more specific? What generation was it?

Jenny: Well, in those days if you were five years older or ten years older, that was already a different age classification.

Maher: So these would be graduate students?

Jenny: And postgraduate students. The people who used to go to his lectures were probably in their thirties or older. So they had a different experience during the war than we had in high school. I think that there was stratification as to whether you were an adolescent during the war or matured before the war. This had quite an effect on your whole outlook.

Maher: Do you think that these were mainly people who had actually participated in the war? Were they soldiers?

Jenny: They participated or had been at the frontiers or were already involved in prewar politics and were more aware of the consequences economically and socially than we were.

Maher: What was the content of the lecture by Hesse?

Jenny: I think the emphasis was on poetry, not on philosophical questions but the beauty of expression and on reciting. This was still popular, to just have an evening of reciting poetry of German, Swiss or French poets, and the auditorium would be full.

Transition to Graduate School in 1923##

Getting a Grounding in Chemistry

Jenny: While the agricultural curriculum exposed us to a broad, educational experience, it did not provide depth in any one field. I got my diploma in agriculture in 1923. But then under Wiegner's influence in agricultural chemistry, I decided to go into research. So when I talked with Professor Wiegner, I said "I'd like to do graduate work in agricultural chemistry, specifically soil chemistry." He said, "Nothing doing. If you want to work with me, you must have a thorough foundation in chemistry." He wanted me to acquire a solid foundation in chemistry and he advised me to go back and study this subject in depth. So I went back to class work and lab work again.

Maher: This was after you had graduated?

Jenny: After I had graduated with a diploma in agriculture. It would correspond to a bachelor's degree in agriculture. This, together with assistantships and all that took me five more years to get a doctoral degree.

Renowned Scientists as Teachers

Jenny: Physics was taught by Peter Debye. Physics was taught by Paul Scherrer, who had discovered the crystallinity of colloidal gold, a great scientific event. Other teachers were Staudinger, the organic chemist, who was fabricating organic thread molecules, and Niggli, the renowned teacher of crystallography, a subject that had been revitalized by x-ray analysis. During a subsequent stay in Paris, I listened to Jean Baptiste Perrin's animated lectures on the behavior of colloidal particles. Later, he (in 1926) as well as Peter Debye (1936) and Hermann Staudinger (1953) became Nobel

Jenny: Laureates. The galaxy of scientists at Zurich attracted guest lecturers from all over Europe and both the Technical Institute and the University--they were on the same "campus"--were beehives of activity and excitement.

Before long, I became Georg Wiegner's chief assistant in soil and colloid chemistry. I was responsible for the extensive lecture demonstrations and supervised the student's laboratory periods. At any time, Wiegner had twelve or more graduate and post-doctoral students from all over Europe and the United States (e.g. Bradfield). He was the acknowledged world leader in soil chemistry. Therefore, our lab turned into a Mecca and it was always overcrowded.

With Wiegner, I collaborated on a modern manual of agricultural chemistry which was for me a mint because I had to perform and explore all the techniques and experiments described therein.

At one time I accompanied Georg Wiegner to a gala science meeting in Leipzig (perhaps in 1922) and listened to such famous physicists as Max von Laue and Max Planck. It was rumored that Albert Einstein was absent because of anti-Semitism. It was the first time I encountered the word and it was a curiosity soon to be forgotten. Also present were such founders of physical chemistry as Nobel Laureates Svante Arrhenius, Wilhelm Ostwald, Walther Nernst and their followers, Herbert Freundlich, L. Michaelis and others.

In these years I had close contacts and friendships with assistants and graduate students from other departments. In a snapshot taken at that time one recognizes Frey-Wyssling (botany), Streckeisen (geology), Signer (chemistry), Kleiber (animal nutrition), Ott (physics) and Biasch (psychology). All of them became professors in Europe or in the United States.

I became interested in soils in nature and began spending weekends in the field and returning with soil samples which I analyzed. This activity brought me in contact with Dr. Josiah Braun-Blanquet, a relatively young but accomplished plant sociologist. We worked together in the Swiss National Park and, after my graduation, we published a book on plant development and soil formation.

Georg Wiegner's Approach and the Rise of Specialization

Maher: You characterized Wiegner as being somebody who was the last of those scientists who could encompass all that was contingent on agriculture. Did you feel that there was too much specialization and that you would never be able to draw things together yourself when you were in the university?

Jenny: No, I had broader interests. In fact, Wiegner told me I was too broad in my interests. Now, Wiegner was a colloid chemist of high reputation. Before, he had been a professor at another university (he was German), he had written and studied about ion exchange and he had worked on the colloid chemistry of milk. On that basis, he became a professor of agricultural chemistry in Zurich. So he looked at all of agricultural chemistry from the viewpoint of colloids--from colloid chemistry or colloid science. He was a brilliant talker and an idea man also. He impressed the educational establishment with his approach. Thus he would give a lecture on soil science in terms of soil colloids, implying that everything else that was done was really old fashioned, and he would lecture in milk chemistry and in wine chemistry the same way. This shows he had a theme that he carried very successfully through his work, including in finding new things.

Maher: I'm interested in getting your impressions of the capacity of a university person to become a renaissance man or a person like [Johann von] Goethe, who could bring everything into his realm and master enough that it was all solid, or did you and your classmates feel by now that this was already a thing of the past?

Jenny: At that time specialization was not as advanced as it is now. Ernst Haeckel, in zoology, also lectured on philosophy. He was a monist. It was still possible to know a lot about physics, chemistry and biology. People like Frey-Wyssling, while they did minute research, were also generalists. I didn't bring it out previously, but during my high school days and before the most famous man was Albert Einstein. His relativity theory was put into popular versions and it was discussed at dinner tables and at social gatherings. I remember that we were at a party once and one guest said that Einstein predicted that the atom is full of energy and that someday people would use the energy of the atom. We thought it was a speculation that had no relevance. What Einstein did was world encompassing. Thus the idea of the cultural conflict we read about nowadays is about specialization versus generalization. It wasn't put into focus the way it is now in that you are either a specialist or a generalist. The two were fused together. Looking back now, I remember I heard about physics professor, Peter Debye, mentioned earlier, whose assistant called him an extreme specialist and an ignorant person who would read only physics and "whodunit" books. But that was considered an exception.

Discovering Albert Einstein and Other New Concepts

Maher: Getting back to your speaking about Einstein, was that all prewar?

Jenny: That was during prewar and post-war years. I ran into the Einstein phenomenon later on because I became a close friend of his son Hans Albert. But we will discuss that later.

Maher: When people discussed Einstein and these ideas, was it mainly taking something in vogue and a nice conversation piece, or do you think that you and your peers appreciated the profundity of the turnover that was going to come forth?

Jenny: No, we had no conception of that. He was the big man with new ideas, startling ideas, which had philosophical implications, and many philosophers were against him. So Einstein was the phenomenon.

Maher: Did the philosophers at your university--the lecturers--make this antagonism clear in lectures or in discussions with the students?

Jenny: No. Are you talking about high school or the university?

Maher: During the prewar period.

Jenny: Oh, during the prewar period. No, I don't think it was discussed in school.

Maher: What gave you this impression that the philosophers didn't accept him?

Jenny: Well, the impression came from what we read in the papers and in pamphlets. They gave the pros and cons and this made an interesting story. People realized that something was going to happen, except nobody knew that it would go that fast. Now they're building nuclear plants all over, and Einstein is barely dead.

Also, in zoology in high school, there were the movements about life forces. The question was "Is there a vital force?" I know there was one guy, a French writer, I don't remember who it was--perhaps Henri Bergson? We read about his "élan vitale." He claimed that in life there is a "vital force." There was also Hans Driesch, the German, who worked on that in embryology. So these things were discussed in zoology, or mentioned.

We had good training in high school. For example, in botany we all had to have a herbarium of 200 plants, collected, pressed and identified by ourselves, with the information neatly printed.

Maher: Do you have any strong recollections of your own reaction to Bergson and others with new ideas?

Jenny: I was pro. Yes, I liked the new concepts. I real Haeckel's books, especially one called, The History of Creation--the famous book where he tore into religion. At that time, in high school, I was still religiously oriented. Our family was religious. My grandmother on my mother's side was very religious. My grandmother on my father's side was Catholic. So I had no such problems, no religious conflicts at that time--except being critical of church establishments.

Maher: You, yourself? Or were the people around you critical?

Jenny: No, the family wasn't. I said we had religion in school and then we had special religious training in the house of a minister, where we had to go, because at the age of sixteen we were to be confirmed and become members of that church officially. We had an old minister, a fuddy-duddy, who instructed us and it just didn't add up. He lived in a beautiful house and all that and talked about things he didn't know about. So he wasn't taken seriously. Especially during the war and at the end of the war with the upheavals, as we went to church and heard what the minister said, well, it was not relevant to what was going on outside of the church. It was just a discourse on some chapter in the Bible.

Maher: So this once against is that sort of contrast that you were experiencing?

Jenny: Yes.

Maher: Maybe this would be a good point to stop.

Jenny: Okay.

III THE RELIGIOUS CONTEXT AND EXPOSURE TO ART

[Interview 13: March 15, 1983]##

Family Religious Influences

Maher: Why don't we start with something about your religious background. You have said that perhaps having matured in Europe contributed to your creativity. Then you said that evidently a lot of creativity and potential was identified in you prior to leaving Europe. Now I am asking if there was an influence from your religious background. When you were in the fifth or sixth grade you were thinking of becoming a missionary. You were a member of the recognized church and "going along with it" until you read Haeckel's work that put a stop to it. But going back earlier, maybe you could start by explaining what the church situation was in your family? What surrounded you from the time you were born.

Jenny: In Basel we had a state church. Basel still has a state church, and let's put it more broadly: I think in Switzerland there are two state churches, one is the Protestant church and one is the Catholic church, and the two religions are more or less divided according to cantons, the political equivalents of our states. So some cantons are predominantly Protestant and some cantons are predominantly Catholic. My family belonged to the Basel Protestant church and they were regular members. I think they believed in all of the dogmas and beliefs of the church, and they practiced it. On Sunday we would usually go to church, certainly on the bigger holidays, as on Easter and Christmas and Holy Friday, and on other occasions. Also, I would go to Sunday school regularly and, of course, in primary and secondary school we had Religion every morning. So there was really no question of the acceptance of this traditional church. Now, I am sure my father and mother didn't think much about religious dogmas, and in their practical dealings they might have thought as we do now that it is "something for Sunday." During the week other things regulate your life. My mother read the Bible, and underlined passages; I still have her Bible.

Jenny: So it was an unquestioning attitude. Also, my parents, especially my mother, stressed the religious aspect because of her upbringing in Meiringen in the Alps. The house of her parents was adjacent to the house of the minister of the church in that village and the families were close friends. So my grandmother and my mother had a positive relationship to the ministers and to the clergy. During one summer vacation it did not prevent us grandchildren (me and cousins) from stealing eggs for fun from the minister's chicken house.

Also, in a church like the one in Basel many of the clergymen were important people who made contributions to society and to cultural activities. They exerted, I would say, a positive influence; there were people like Karl Barth, who later became very famous, also Edward Thurneysen. There was a routine relationship, I would say, with the church, and to us religion was an uneventful thing.

Maher: On your mother's side, wasn't there a Catholic strain in her background?

Jenny: No, that was on my father's side. My grandmother--an early widow--was Catholic. But my father was brought up as a Protestant, which caused conflicts and probably set the pattern for him not to be active in church or involved in the church, although he would do whatever is the duty of a church member. So I would say I had a positive, unquestioning feeling about the church until I went to Zurich, became a student, and was exposed to other ideas and views.

Maher: Were you exposed to the arguments of the day?

Jenny: No, as far as I recall, there was never any religious discussion or controversy between my father and his mother. The problem was with his mother because the Catholic clergy did not approve that she let my father be brought up in the Protestant faith, and I remember that late in her life she finally became what shall I say?--freed--because she said once that the clergy had forgiven her the sin that she had committed.

Maher: How old were you then?

Jenny: How old was I? I was a student then. I think I was in the university.

On Being a Protestant

Maher: What did it mean to you to be a Protestant in Switzerland?

Jenny: [pause] As Protestants in Switzerland there was always a hidden tension with the Catholics. To keep Switzerland together we had to accommodate and make tradeoffs and not fight each other. In the Protestant cantons, there were always a good number of Catholics. We looked down upon them and their behavior did not impress me. Looking back now, I think it was the normal behavior of a minority that would not fight, or simply had to be quiet. That's the way I see it now, but as we saw it then, the Protestants had overcome the Catholic religion with Luther and Zwingli and we were in a way "above" the Catholics. The Catholics, I am sure, thought the opposite.

Maher: How did you react personally? You have given the context for reacting. Now, how did you actually react?

Jenny: You see, for example, in our boy scout troop we had one Catholic, and eventually he left because the Catholic church established their own scout troop.

Maher: What was your view of that person being in your midst?

Jenny: Well, I hated to see him go, but I think I projected my prejudices I had upon him.

Maher: Explain that.

Jenny: I thought he was not fair and open.

Maher: Do you mean his general way of behaving in the troop had been, as a pattern, that he was not fair and open?

Jenny: Yes, but that may not have been true at all.

Maher: Could you give an example of how you had thought of him that way--wrongly or rightly?

Jenny: Well, we never mixed socially with Catholic people. We would mix only with Protestants, and the way my Catholic grandmother behaved also supported these ideas. She used to say to me, "Right you are, but you should keep your mouth shut."

As I said, this was my father's mother who was Catholic, but not my father. This is still a common thing, this tension between the Catholic church and the Protestant church. One of my best friends, a geologist, a Protestant, applied for a professorship in a Catholic canton and he told me he was turned down because he was not Catholic. Where you have a Catholic university it is a different thing.

An Interesting Paternal Grandmother

Maher: How many of your grandparents were alive when you were growing up?

Jenny: Except for my father's father, they were all alive. The two grandmothers were still alive when I left Switzerland and came to the United States.

Maher: When did you father's father die?

Jenny: Before I was born.

Maher: Were you very close to your father's mother?

Jenny: As a youngster, once a week I spent a day with her. I went to her home and we played games and she always fed me a lot of sweets, and I loved sweets. She just dished them out, and everytime when I came home I was sick! I had to vomit and this was a regular thing.

Maher: Were you close to her?

Jenny: No.

Maher: What did it mean to you? Was it like a duty? Did you feel you had to go?

Jenny: No, no, I liked to go, but we just played games. No, there was nothing special. She lived in a very old house, a very interesting house, a medieval house. This was interesting.

Maher: Did you explore it?

Jenny: Oh, yes, I explored it and I made sketches and drawings. I might still have them. For example, the house was not connected with the city's sewer and water systems so there was no running water in the house. We had to walk half a block and go to a fountain there, a public fountain. It was a very interesting thing. It was so different from the way we lived (we had a maid) so, yes, I liked to go but not because I had a relationship with her. It's more her environment and her style and all of the religious pictures she had. I used to look at Catholic magazines and at all of the religious pictures. It was a different world, so it was interesting.

Maher: About how old were you when once a week you were going over to her place and she was stuffing you with sweets?

Jenny: Oh, maybe in grammar school.

Maher: Ten.

Jenny: Less than that.

Maher: Eight?

Jenny: Or six.

Maher: Then continuing up until what age?

Jenny: Oh, I think maybe through grammar school. I'm not sure.

Maher: Maybe six years in total. What kind of games?

Jenny: What kind of games? There were the games where you--there was one with a figure of a king and then inside it had a spiral hollowed. Then you put a marble on top of it and the marble rolled down and ended in one of many numbered holes. If you got the highest number you won. It was that kind of thing.

Maher: What was her style? You mentioned that her style was interesting to you.

Jenny: Her style?

Maher: I think you used that word, yes.

Jenny: The way she was dressed, for example, and she prayed a lot, and the house was very interesting.

Maher: Even though she didn't mean anything to you personally perhaps, did it mean something to you to be able to go to a place where there was play?

Jenny: I can't answer that. But one thing it did: I developed a positive attitude toward Catholic practice, not the politics of the church--I didn't know about that--but the regular routine, the praying in the churches. So I always enjoyed going into a Catholic church.

Observing Values in Other Religions

Maher: What did you enjoy about the Catholic church? Why?

Jenny: Because it was just the opposite of the Protestant church. In the Protestant church you eliminate all of the tangle and all of the paintings and all of the sensual impressions, and in the Catholic church they were there and people seemed devoted and they acted more visibly to religious experiences. So I still like to go to a Catholic church. I wouldn't want to be part of it, however.

Maher: Later, when you spoke about discovering the aspect of your Protestant family branch, the Mennonite, you said that one of the things that impressed you about it was that they incorporated sensual expression through singing, and music.

Jenny: Yes. Well, music was also in the Protestant church. There was always the organ.

Maher: But there was something about the Mennonites that impressed you.

Jenny: Yes, and I would say it was, what is called here, the community fellowship. The Protestant church, the state church, is very formal. You don't know anybody. You sit there and you don't talk to anybody, but with the Mennonites, you shake hands and embrace and it is a community jubilation. In the Protestant church, the minister would give a highly erudite speech which didn't mean a thing to most people.

Maher: Was it also this expression of community that you were finding in the Catholic church and at your grandmother's home?

Jenny: A community, yes, but not because of people. I didn't know any other Catholic people. No, there was isolation in the Catholic church also. People went in and didn't talk to each other, but because of all of the pictures and the statues and a lot of excellent old art, there was an appealing atmosphere.

Ernst Haeckel's Writings

Maher: Later, you read Ernst Haeckel, when you were in the first couple of years at the university.

Jenny: Yes.

Maher: That seemed to have a really marked effect upon you. You tend to refer to that very specifically. Why?

Jenny: Yes, and I remember the way it came about. I went to the university the first year after World War I. I remember that during the war we were pretty well shaken up by all of the killings, and the question came up: How does this conform with religion and with claims made by both sides that "God is with us"? It created a base upon which the Ernst Haeckel book really "put the dot on the i"; it really opened up the fact that this whole religious question was not as simple as I had imagined.

Maher: The religious question?

Jenny: Yes, in considering "Is God loving us all?" If that is so, we ask, "Why does he have these innocent people killed?" and answers were not given. There was an answer as to what the whole thing was about, from a biological perspective.

Maher: Do you mean that it came from Haeckel?

Jenny: Yes. He was a biologist and monist philosopher.

Maher: What was the answer?

Jenny: He questioned this idea of the soul and immortality, and this made sense to me, that you had to inquire, "What does this whole thing mean? Can it be explained by our behavior and in terms of the religious dogmas?"

Maher: It strikes me that the position by Haeckel was mainly a negative one; that is, pointing to inconsistencies and pointing out the absence of things like immortality or the soul. Is that correct?

Jenny: Yes, I don't recall that I came out with a positive replacement or a substitute idea.

But Haeckel's book exposed me to the theory of evolution, and I was captivated. It led me to reading Darwin's Origin of Species. One summer day I was lying in the grass at the edge of a forest looking at the clouds, and thinking: all these trees, the grass, and all the animals, and me, are all related. It made me feel good. Today, I would exclude the lions, bears and rattlesnakes.

Maher: Just as you were talking about the war, both sides were claiming they had God on their side as it were, in the context of religion or churches, both Catholic and Protestant churches were saying that they had the answer to what God was--

Jenny: That's right and the churches on both sides supported the war.

Questioning Religious Dogma

Maher: Did it strike you that there were two groups that were trying to get answers to the same thing and that neither one was succeeding?

Jenny: No, I didn't see it that way, but I remember in Zurich, for example, I still attended church services on Sunday, and I started to argue with the preacher. I wrote him a letter and argued with what he said. There was also a religious group attached to a famous

Jenny: churchman by the name of Leonhard Ragaz, who questioned what the established church was doing in connection with the war and the social upheaval afterwards.

Maher: Okay, now in your arguments with the church, what happened?

Jenny: Well, I would say, nothing.

Maher: You wrote a letter and get no reply.

Jenny: I got no reply.

Maher: Okay, now, what you were writing you characterized as argument. What was your stance? Were you primarily pointing out inconsistencies or weaknesses?

Jenny: No, I was searching for something and I remember I wrote to him--now, I don't know why I did that--and noted "In your sermon you gave us stones instead of bread." So I was looking for something. There was this upheaval with Haeckel and I was groping for something and it did not come from the minister, although he was a famous churchman with a Ph.D. and whatnot, very scholarly and all that, but--

Maher: Why did you say "you are giving us stones instead of bread?" What is the meaning of that?

Jenny: I don't remember what it was, but I remember that is the phrase that I wrote. Well, I couldn't eat it. I wanted something to eat and I couldn't swallow this.

Maher: You meant eating in a figurative sense as spiritual nourishment?

Jenny: Yes, what he said in his sermon was just stones. It wasn't digestible; it wasn't helpful; it wasn't nourishing.

Maher: The image has more meaning to me that you chose stone as opposed to let us say wood. A stone is something hard and cold.

Jenny: Yes, I would never use wood for something negative because of my background in the wood carving factory and the wood carving experience. Wood is a positive thing. Stones are not, but a geologist might not agree.

Maher: So do you think that the effect of this was that you were exposed to the inadequacies of the two churches, the Catholic and the Protestant, and that you were still trying to search for something else?

Jenny: Yes, but also during the war, the German Protestant church and the French Protestant church claimed God was on their side and the German Catholics and the French Catholics claimed God was on their side. So it was a political division.

Maher: But I think that it is important to delineate that your reaction was a partially positive one in the sense that it was still searching for satisfaction, still searching for "bread," to use your image.

Jenny: Yes, and I didn't give up the church. In fact, when I came to the United States, I went to church. My wife and I joined the church after we were married and even in Berkeley we were active in the church, so it was not that I became an atheist. It was more that I was skeptical of the religious institution.

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Maher: Now, I think my line of questioning should be obvious. I am trying to learn from you whether you were being stimulated--by inconsistencies and inadequacies, as contrasts and as different worlds--in order to come up with more fundamental approaches to the same question.

Jenny: Yes. This goes into psychology and the subconscious. I cannot answer that. I don't know. I am not able to sort out all of this and attribute my behavior to various ideas, but maybe someone, maybe a psychiatrist, by listening to this, could pick out a trend. I don't know. I haven't read anything about what makes people creative, as I am supposed to be.

More on Family Influences and Creativity

Maher: You have said that you recognized that you had a certain amount of creativity early on. How would you characterize the household that you grew up in in Basel with respect to creativity?--play, fun, expression, art, sensual features.

Jenny: My father and mother had a lot of conflicts, but they also had a lot of things together. As I said, my father played the violin. My mother played the zither and sang and they played together a lot. My mother also was what you would now call a "women's liberationist." At that time it was much more modest, but she subscribed to women's newspapers. My father also read them. I know they went to concerts and talked about it. When they went to an opera, they would sing the melodies afterwards. As far as I recall, there were not any philosophical discussions. They were not what you would call intellectuals, but I would say my mother had creativeness. She

Jenny: would paint and do things with her hands, make the home attractive with flowers. Yes, it was a positive thing. Father, mother, sister and I did much walking, hiking and picnicking together.

Maher: Was it a joyful environment?

Jenny: It was joyful except for the conflicts between my parents. The conflicts had to do with money, and, I think now, also because of my mother's feminist viewpoint. She would rebel against certain traditional things my father would do or expect.

Maher: Like what?

Jenny: I don't know, I don't know now. Yes, I know one thing. For example, he insisted that she keep track of every cent of expenditures in the household and she couldn't see the value of it and they had arguments about that.

Maher: Did she insist that he keep track of all of his expenses outside?

Jenny: I don't know if she ever knew how much money he made. That was not revealed in those days. There is one other thing. A woman, when she marries, gets a dowry from her parents and the money which she has goes to the man. So my father had control of the money she brought. So there were frequent conflicts and I did not like it.

Then my paternal grandmother was involved. Now, she, of course, had been a widow a long time, and I guess the money ran out. Everyday she ate with us or she spent the day with Father's uncles and aunts; she took turns during the week. She told everybody what the others had said and there was often a commotion and arguments. It was an impossible situation.

Maher: So that would be one day a week that she would be visiting you?

Jenny: Yes.

Maher: Was it a regular day?

Jenny: Yes, I think it was a Wednesday that she came to us.

Maher: Were those arguments swept under the rug or kept down and out of sight?

Jenny: No, they were out. I would say the whole thing disturbed me. Of course, my very favorable impression of my religious relatives in Zurich was that they had this seemingly very peaceful family life with no arguments whatever. I relished it.

Discovering Mohandas K. Gandhi and Albert Schweitzer

Maher: You were pretty young when you first read about Mohandas K. Gandhi?

Jenny: No, no, Gandhi was, I think, in his prime when I was in high school and at the university.

Maher: How did you hear about him? Did you read Romain Rolland?

Jenny: Yes, and there was the continuing struggle with the British, and the newspapers were full of it.

Maher: Was there something at that time that particularly impressed you about Gandhi as contrasted with your view after you went to India?

Jenny: Yes. The way the books represented his struggle--it was a struggle with England, but it was not presented as realistic political dealings. He wanted India to become independent and the means he chose were really the only means he could afford without causing a lot of bloodshed. The struggle was represented as one of religion and philosophy, on a high plane, rather than the concrete daily actions of how to get rid of the English, and I didn't know that until I went to India.

Maher: Okay, let's take this time when you did not know about the practicality, the practical politics. Was there an appeal to you in Gandhi?

Jenny: Yes, yes, Gandhi was also an answer to the postwar situation. There was enormous turmoil in Europe. Solutions had to be found on a peaceful basis, and that is what he advocated. But what I didn't know was that this was the only thing he could do because he didn't have the arms to fight a war.

Maher: Yes, I'd like, for the time being, to focus on what you thought of Gandhi at that time.

Jenny: I thought he had a new message to help overcome the problems of Europe, the war situation, how nations could get together.

Maher: Did you read about him before or after reading Haeckel?

Jenny: I would think at the same time. Another famous person, Albert Schweitzer was also active at that time. Just at that time, he was starting his hospital in Africa in Lambaréné.

Maher: If this was a time when you were questioning the tenets of established churches, why was it that someone like Albert Schweitzer or Gandhi had an attraction to you since they were both religious people?

Jenny: There were the official politicians and the official leaders like the Kaiser and the great generals of England, the Czar in Russia and there was the Bolshevik revolution; the so-called establishment had been discredited. So we were looking for new forces, and here there were two guys.

More on Haeckel's Impact

Maher: Do you think Haeckel was looking for new forces?

Jenny: No, Haeckel wrote his story before the First World War. He was a zoologist.

Maher: Do you think that reading Haeckel's views was satisfying to you? How did you feel upon reading Haeckel?

Jenny: I felt that he gave an explanation of many contradictory things.

Maher: Was his explanation primarily that there wasn't anything? Was it an atheistic view, Haeckel's view? Were they arguing over something that didn't exist?

Jenny: For example, he discussed a lot about the soul and he said we should be able with an experiment to get the soul and condense it at low temperature and get a soul snow. This is the way he argued these things.

Maher: He was being facetious, constantly?

Jenny: Yes.

Maher: How did you feel about that?

Jenny: Well, it was just news to me. I had never thought about these things, but I began to realize what may be meant by the soul. I had accepted everything completely that you had a soul and a heart, and that the soul made the difference between man and animals. He showed something about the modern socio-biologists bringing in more of the biological functions, and that's what Haeckel did for me.

Maher: Now, in your reaction to Haeckel, what were you defining as the soul?

Jenny: I simply left it as a question mark.

Maher: Did that satisfy you? If you say you "left it," that sounds as though--

Jenny: I didn't accept the old idea of the soul. I didn't know whether Haeckel was right or not. I was not sure that he was right, but nevertheless, at least he questioned some of the basic concepts which I had accepted as religious dogmas.

Maher: I am a little bit uncertain about your reaction. You were a person at the point where you realized that so-called explanations or definitions were wrong or inadequate. But there is a difference between saying "Okay, that is all nonsense and there is no reason to pursue it any more," and, having shed those now thought as wrong ideas, then pursuing the same questions anew. Which do you think you were doing?

Jenny: I would say I was too young to do what you thought I might have done. I think a more mature person might struggle for a philosophical system, or formulate answers to the big questions. No, don't forget, in Zurich I was also exposed for the first time to girls, to interesting lectures, to serious mountain climbing. There was a whole spectrum of activities going on, and I did not shut myself up in a cell to ponder.

Maher: But you did continue to go to church and you did write a letter and you did have a positive response to Schweitzer and Gandhi rather than just pooh-poohing them--

Jenny: Yes, that's right.

Maher: Were Gandhi and Schweitzer of some interest to you in that they were doing something?

Jenny: Oh, yes, yes, I would think so. Yes, there was no question that Gandhi made serious trouble to the English. At that time, independence was still far away. I guess independence came after I had arrived in Berkeley, in the forties or so, so it was a long episode. So the Gandhi cult came up at the early stages of the Indian struggle.

Maher: Did you hear anything about Gandhi when he was in South Africa?

Jenny: I read about it.

Maher: In the newspapers.

Jenny: I guess in the books about Gandhi.

Maher: You read about it <u>after</u> it happened?

Jenny: Oh, yes, after it happened.

More on Gandhi: The Trip to India

Maher: Okay, now let's just skip ahead. This is out of context, but I would like to skip ahead to when you went to India and you got a new slant on what Gandhi had been involved in. You felt then that what he was doing was to choose the only means available to him.

Jenny: That, I felt, was the main motivation for what he did.

Maher: Because he was powerless?

Jenny: Yes, he was powerless in the sense of physical power.

Maher: So he chose the only thing remaining, which was nonphysical power.

Jenny: That's right, nonviolence.

Maher: Why do you think he chose not to accept independence if it meant the division of India between the Hindus and the Moslems? If he wanted independence at any means, why did he reject independence if it meant division?

Jenny: He was for strengthening India and my understanding is that the conflict between the Moslems and the rest of India was really exaggerated and pushed by the British. That wasn't there when he started. They all considered themselves Indians, although they had differences and fought each other and all of that. But that division was created by the British. That was what I learned, somewhat from talking to Indians. At that time, I knew quite a few Indian students.

Maher: Were they Hindus?

Jenny: Yes, Krish was a Hindu, but I also talked to Moslems, they were together, Krish and Sengupta, two Hindus, and then there was the Moslem, and they were together.

Maher: But if independence was what Gandhi had wanted and he was going to choose any means, why didn't he choose it then? Why did he postpone it? The British were going to give it to him after agreement on the division, but he refused it. Why? He would have had independence. He already had the power.

Jenny: By separating India, by dividing up India?

Maher: Yes. Why did he refuse, in your view?

Jenny: He must have felt it would lead to bloodshed. I don't know.

Maher: There was an earlier time, I think about fifteen or twenty years earlier around 1935 perhaps, when he really had a tremendous advantage and yet he stopped. He pulled back his campaign. He could have gotten independence at that time. He was forcing the British into it, but he stopped. He said the time was not right.

Jenny: Yes, but I assume that he wanted a united India, not to divide it up into two nations like it is now, Pakistan and India and Bangladesh.

Maher: How about during the war? During the Second World War, many people felt that it was obvious that now was the time to press for independence but he said, "No, on the contrary, now is not the time to press because the British would give us independence but the British are at a weak position." If the main thing he wanted was independence, why didn't he push for it? Now he had this leverage that the British were preoccupied elsewhere. Why did he do the opposite and not press?

Jenny: He must have given answers to that, but I don't remember them.

After I came to the United States, my interest in Gandhi was not as strong as it was in Europe. There were too many new things impinging on me. I read about him, I was in favor of him, but I had too many of my own problems, interesting ones. I had a family and I had problems, the Depression, and whatnot.

Maher: The economic depression?

Jenny: Yes.

Early Exposure to Art and Nature

Maher: Okay, we will leave that. There is only one more area that I want to pursue if you don't mind. What can you recollect about being involved in art prior to high school. You mentioned drawing at your grandmother's house in Basel.

Jenny: Yes, but much earlier, in Meiringen, in the Bernese Oberland, we had the wood carving factory and I had two uncles who were artists. We were together and they talked about how art was the thing and I think that is where some seeds were planted.

Maher: That was when you were about eight.

Jenny: Let's see now, no, I was older. I must have been ten or more. I was in grade school in Meiringen. We had the large store with all of the wood carvings and the tourists came and admired it. I was there and helped with little things, so there was always a positive

Jenny: reaction. I remember the uncles would draw. Once, for school homework I had to draw a flower for a biology class; an uncle would draw it too and it was just wonderful.

Maher: You would have been going to your grandmother's house prior to having gone to Meiringen--your father's mother.

Jenny: Before going to Meiringen? Oh, yes. But also afterwards, occasionally.

Maher: So your sketching at your grandmother's house perhaps preceded--

Jenny: No, the sketching I did at my grandmother's house in Basel was afterwards; I was already probably in junior high school. It was an old house, now an historical monument.

Maher: So you are pretty clear that as far as art was concerned, the real surge for the practice of it was from the time of being in Meiringen.

Jenny: But, as I said, my mother did art work in wood carving and painting wood, so I cannot give a date.

Maher: Did she encourage you to join in with her?

Jenny: No, I don't recall that.

Maher: Was it really a strong influence? Was she always seeking time to do her art work?

Jenny: No.

Maher: It was just occasionally.

Jenny: Also, there was a period when we had financial difficulties and she would get unfinished wooden boxes then burn the wood with a needle and paint the boxes and sell them. [pause] So I can't pin it down.

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Maher: Insofar as your exposure to nature is concerned, natural bodies and so forth, the main thing that I have in your background is the Boy Scouts and hiking. Now, can you go back further than that?

Jenny: Yes, I can go back further. I'll show you an old photograph I just got. [tape interruption] Here is the view from the house in Meiringen.

Maher: Beautiful.

Jenny: This is the factory where the wood carving was done. Here you see the water pipe coming down the steep rock cliffs. It furnishes the water to drive the water wheel. Now, as kids we would climb around here, rock climbing, and looking for flowers and making carnation bouquets. This was really a challenging site for climbing around.

Maher: It's very steep.

Jenny: Also in wintertime, or in springtime, we went way up here skiing. It was all outdoor work, and we boys did a lot of mountain climbing. We went for a day or two at a time. So, besides the farm--and the farm was right here. Here was the house and here was the farm. So I was exposed to farming but also to nature.

Maher: Now, you identified the time you went to Meiringen as when you were about ten, right?

Jenny: Ten or twelve. I was there three years, I believe. Back in Basel, I remember the sinking of the Titanic [1912], I had scarlet fever at the time.

Maher: Yes, and when did you join the Boy Scouts?

Jenny: Afterwards.

Maher: At what age?

Jenny: Let's see now--I remember for sure I was in the scouts when World War I started. We were on the first of these three-week hikes and the question was "Can we get home before the war starts?" We did.

Maher: So that was in 1914?

Jenny: It was. Perhaps I joined a year before that.

Maher: You have in your notes that you went to Meiringen between 1908 and 1912, so it means that you were about nine when you went to Meiringen. Is that correct? Okay. But then prior to you being nine there was nothing. You were in Basel but what about when you were younger than nine? Did you go out into nature?

Jenny: Yes. In what is now France, in Alsace-Lorraine--at that time it was German. Right outside of the city of Basel, there were extensive swamp areas and we used to go there. We walked there all day Saturday, and walking meant looking for frogs and snakes. There was a lot of wildlife and we enjoyed that, but it was curiosity-type exploration. It was nothing scientific, but since you bring it up now, yes, it exposed me to nature. I went mostly with my cousin Arthur, but he never developed any interest in nature, as far as I know. Nor did I at that time.

Early Creativity

Maher: When you were in high school, you often thought of mediocrity as being worse than a crime and you viewed yourself as mediocre. You said that you had a lot of strife around then and that you couldn't memorize well. Did you look forward to accomplishing much? Did you think that you were going to accomplish much in your life?

Jenny: No, because the things that turned out later to be valuable, namely having ideas, was not valued. What was valued was memorizing stuff. [tape interruption]

Maher: You were definitely not being encouraged by the standard values to be creative or to have ideas. You were encouraged to memorize languages. You memorized Latin and Greek and math was also mainly memorizing formulas.

Jenny: That's right. Now, what I did fairly well was writing.

Maher: Creative writing or the grammar part of it?

Jenny: No, writing themes, term papers and things like that.

Maher: Yes, you mentioned the one about the English painter.

Jenny: Oh, yes, but that was in English. It was more difficult. But I liked to write in German and I received good notes.

Maher: Did you have confidence that that was something of value in you--to create in terms of ideas, to come up with a theme, to come up with a written piece, that your mind had created?

Jenny: No, I was unaware of these things. But I remember taking an exam to be a scout master or something. We had to write something on the theme: "Suppose you are standing with scouts on a bridge over a railroad track. What would you tell the boy scouts?" So I wrote and apparently poured out the most ideas as to what to tell the kids. So apparently I was wide awake, but not in the view of my old school teachers. In fact, we had no young school teachers.

Maher: Did you begin to recognize that you wanted to find out--did you begin to recognize that you wanted to do something that would be giving you a chance to express yourself?

Jenny: No. [pause] No, I think I was pretty well down.

Maher: What do you mean pretty well down?

Jenny: Put down by everything.

Maher: But even if a person is put down, one can in a sneaky way seek an outlet.

Jenny: I may have done that in some of my drawings and in my activity with the scouts, but was not conscious of it. I was very successful with the scouts and I still have my drawing books that I did then, my sketchbooks.

Maher: Yes, I was glad to hear that because, going back over the material of the scouting period, it was clear that it meant a lot to you because you were getting positive reinforcement for what you were doing. You were saying, "That's great Hans," and that was in contrast to what you were getting in high school. But it struck me that that was mostly for things that you accomplished with your hands. Now, this is, I guess, maybe the first time to realize that for thinking things out or for having an idea or something like that you were also getting a pat on the back.

Jenny: I think most people underestimate the mental effort involved in sketching or painting a landscape. Unlike taking a snapshot, a sketch cannot record everything you see but must discriminate, select and delete, reduce and exaggerate, which calls for creativity.

Remembering Meiringen

Jenny: Here is a picture of "Althaus"--my mother's name for our Meiringen home. [Jenny shows a photo of the Meiringen house and its natural surroundings]

Maher: Althaus: is that "high house"?

Jenny: No. "Old house."

Maher: Did your family go back a long way in the Meiringen area? You mentioned that you felt a certain prestige there.

Jenny: Yes, they had a factory there where they got special kinds of wood and cut it up and then every weekend the farmers came and picked it up and carved it. The factory workers made frames and boxes for sale and the farmers would draw and paint them.

Maher: So the Swiss farmers were not able to meet their livelihood needs by farming alone.

Meiringen. Althaus where wood was cut for carvers; frames and boxes fabricated - ca 1905.

Jenny: Oh, no, in Haslital they had to have cottage industries such as wood carving. My grandparents, because they hired people, were a big force in that valley. In this photo, that is the cliff wall on one side of the valley, and there is the valley and then a few miles over the valley's opposite wall goes up.

Maher: Is it really precipitous?

Jenny: Yes, and I remember during heavy storms big boulders would come tumbling down the precipice and it made a hell of a noise. You couldn't sleep at night!

Maher: That I would have loved.

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IV RESEARCH ON COLLOIDS AS A GRADUATE STUDENT WORKING WITH GEORG WIEGNER, EARLY 1920s TO 1925

Wiegner's Teaching and Views on Science

Maher: Do you remember when you first met Georg Wiegner? What was that like?

Jenny: I think I met him first in the classroom when I took his course.

In the upper division I attended the courses on agricultural chemistry by Wiegner which dealt with animal nutrition, milk, wine chemistry and soil science. In the latter course I was spellbound from the beginning because Wiegner expounded principles, concepts, and ideas, rather than practical recipes.

[Interview 3: April 9, 1979]##

Maher: In the notes you and C.E. Marshall made about Wiegner, both of you have repeatedly mentioned that being in contact with a man like Wiegner was like being under his spell--the magic of his personality, and that sort of thing. What was it like to be with a man like that? What was he like that made such an impression?

Jenny: He was a dynamic person and an outgoing person and his mind was very active. He commented on everything. He was an excellent lecturer, and also humorous. He always had many experiments going on, demonstration experiments for the class. He painted a perspective about soils I had not known before.

Maher: So he was one of the teachers in Zurich that you mentioned who were tying their lecturers together with actual research.

Jenny: Well, with broad conceptual schemes. As I see it now, in soils, he had to do that because he was a chemist who had no practical agricultural experience, yet he had to teach a general course in soils, and it could not be only soil chemistry, which he was primarily interested in.

Jenny: There were a lot of technical application books on soils, dealing with a drainage problem, a cultivation problem, how to construct a good plowshare so it would turn the soil correctly, how deep you should plow, or how often you should plow--all of these very practical questions. Well, they didn't interest him and he had no emotional relationship to them. He had never walked behind a plow. To him the soil was an object in nature. He could apply intellectualism to it and speculate to see what he could do with it, in his line, and Wiegner did it very effectively. His lectures were eye-openers, but the interesting thing was (I didn't know it at that time--only after I had become his assistant and worked with him) that he talked about all of these great international soil groups, like chernozems, and podzols, and sodium clays--all of these things, without ever having seen them. This was all book knowledge on his part. But it fit into chemical equations and into conceptual schemes--what atoms would do, what colloidal particles would do, and he was always emphasizing that in soil, they were doing the same things they were doing in milk, or in any other natural body.

Maher: After discovering that, did it strike you that maybe other areas of his very broad, general knowledge were mainly dependent upon book learning, things that he hadn't actually seen or put his hands to himself?

Jenny: Oh, yes, especially in another area in which I was not very much involved and that was animal nutrition. My friend Max Kleiber, who used to be at the University of California at Davis, brought that out many times, that Wiegner had no practical background. But I would not use the word "book learning." I would not apply that because it was much more than book learning. Sure, he took some facts or descriptions out of the books but then he integrated them with what he knew and with recognized chemical principles. So it was a book learning that had gone through the mill of thinking and been digested, so to speak. So it was a new thing which he offered, and I think that did not leave the impression that he had just copied or studied books. No, it was a vivid thing that he presented.

He had to be broader, and, since he hadn't experienced the practical problems in soils, he had to search for other things that he could grasp and develop. These were the conceptual schemes and especially those of the Russian school.

Maher: I was going to ask you to briefly sketch the view on soils work that Wiegner inherited in that way--from the Russians and the other Europeans perhaps.

Jenny: Well, one of the prominent books in those days was one by [E.] Ramann. In 1914, [Constantin D.] Glinka's book on the soils of Russia was translated into German. Wiegner became captivated by the Russian climatic principles of soil science. In combination with [Eugene Woldemar] Hilgard's climate paper of 1892 (which also appeared in German) and Hilgard's book on soils (1914), Wiegner put together a soil science course that was ultramodern at that time, and essentially non-Western European. Here was something new. Students from a wide variety of disciplines attended the lectures and were deeply impressed, not knowing of course, that Wiegner was sketching a one-sided picture and sometimes a wrong one, especially in his ideas on the physics of soil moisture. Nor did I myself appreciate these deficiencies until I came to California.

Wiegner fused colloid chemical principles with soil genesis and explained the classic profiles of podzols (spodosols), chernozems, (mollisols) and natrium (Na--sodium) soils. In this attempt he was helped by the translations of [K.K.] Gedroitz's colloid chemical analyses of Russian soils. In 1918, Wiegner integrated his ideas in a slender book titled, Soil and Soil Formation In Colloid Chemical Perspective. This charmingly written account reads like a novel and proved to be an immediate and long-lasting publication success.

While Glinka's and Gedroitz's books were eventually translated into English in the late 1920s by [Curtis Fletcher] Marbut and [Selman Abraham] Waksman, Wiegner's jewel was not, and American soil science was exposed only to Sante Mattson's "Laws of Soil Colloidal Behavior" that appeared in numerous issues of the journal, Soil Science, during the 1930s.

Because of his unusual approach, Wiegner made an impact on soil scientists in all countries and he worked with large numbers of doctoral and postdoctoral students from all parts of Europe and from overseas.

So it was very stimulating. In scientific discussions he always put out concepts or controversies or even became personal about other scientists, elevated them or put them down. But everything was personal. We were all involved as persons. So it was not neutral territory where you put up one thing and balance it from the other side and see how it comes out. The whole thing was really, I would say, emotional.

Maher: Did he make science a living experience?

Jenny: Well, it was actually all intermingled--objective science and personal feelings. There was no so-called "objective scientist." They didn't exist in his thinking. When I said that he was personal in his judgments, I was thinking of a fellow in Breslau in East Germany who was not a pure chemist, but also a colloid soil chemist, so to speak. His name was [P.] Ehrenberg and he wrote a book on soil colloids. Wiegner denounced him as not being really qualified. But that was a one-sided view. For example, he also criticized the great Russian colloid chemist, Gedroitz, who had an enormous practical experience with soils, who knew and worked out ion exchange reactions and ideas of exchange capacity and colloid interactions. Wiegner used his work and spoke favorably of him but nevertheless he didn't consider him an overall creator of concepts. He felt he was too much involved in the small details. But it may simply have been professional jealousy or something else. At that time I hadn't been in science long enough to realize how big a thing this is in science--whether you recognize another fellow or not. I think it depends a lot on what you think about yourself.

Maher: By this criteria, how do you think Wiegner was in contrast with other European scientists?

Jenny: I don't know. Except for one individual, I didn't know other outstanding scientists closely enough to know that. Now, of course, I can think about it. Some of my old friends have become professors and so I know how they behave and what the problems are; also, I learned at Berkeley what goes on in academia.

Maher: Did that play a really big role in what ends up in classrooms as being taught and what is in books and the overall concept of soil science?

Jenny: [pause] I don't know. I did not think about that in broad terms. In the field, I was really interested in the colloid chemical contributions to the soils.

I should say that as students in Europe, we were more critical of the professors than I have experienced here.

Altered Concept of Wiegner after Work in the United States

Maher: Was Wiegner aware of the kind of contradiction between being a scientist and getting into all of the controversy and subjectivity?

Jenny: Yes, I'm sure he was aware. I would say, however, that the idea of the abstract scientist who searches for truth--I encountered that notion in the United States. In Europe we never thought about it in that way, although the idea of doing truthful research--trying to be

Jenny: objective in research was paramount--but certainly not trying to be objective in setting up what you want to study; this was always related to some other thing. The prevailing slogan we had was that "facts mean nothing"; facts are boring unless there are theories behind them or money.

Well, I had one period during which I got tired of that. For a while I shifted my loyalties to another professor I had by the name of Max Düggeli. He was a bacteriologist and he was only interested in knowing what this bacterium will do and that bacterium will do, and there was of course all that time no biochemistry behind it. It was just factual material and you didn't think about it. So maybe I had to take a vacation from speculations!

Maher: When was that, that you made this shift?

Jenny: Well, I think after I came to the United States.

Maher: After you came to the United States and then returned to Europe?

Jenny: Yes, when I went back for a visit it became very obvious to me that I had changed. I explained it to some of the Wiegner people who were there and still talked about Wiegner the way I used to. Other people who put emphasis on measurement and not on theory appealed to me for a while, so I guess gradually I had a sort of fusion of ideas. This has to do with a point that should be discussed later, when I was confronted with science in the United States and especially soil science having a different emphasis. So that was a reaction. Soon after coming to the United States, I realized that certain of Wiegner's ideas were onesided, his own or the European ones, because of a different historical concept of what we want to do and what science should do.

Georg Wiegner as a Person

Jenny: He was born in 1883 in Leipzig (now in East Germany). He was raised there and graduated from the university as a chemist. He then studied colloid chemistry with the great German chemist, Richard Zsigmondy in Göttingen. In 1913, his research on milk and soil chemistry landed him a professorship in agricultural chemistry in Zurich. During the war, he served in front of the English in the trenches and was wounded.

I don't really know why, but even about Wiegner--we had our little stories on his idiosyncrasies or little habits that we laughed about. But in a way, that did not cloud our impression of him. It was the sort of thing students always did.

Maher: What were some of the stories?

Jenny: Well, for example, he had a physical handicap in that he took a bath twice a day because of his sweating. He had quite a body odor. So that was discussed by way of a joke, or we talked about what he ate and what he liked, little things--or whether he was a henpecked husband, or stuff like that.

Maher: Was he?

Jenny: Yes, I would think so. I knew his wife quite well. However, it was more, I think, a division of labor and he assigned her certain territories which she would take care of.

Maher: Did he assign them or did she assign them?

Jenny: Well, I would say it came naturally. His world started the moment he stepped out of the house and it ended as he entered the house. So there's a physical division.

Maher: [laughs] How did you and the others react to that?

Jenny: Oh, we laughed and we made remarks, but it was all in good spirit. But as to making such remarks, I haven't experienced that the students do that here.

Maher: It's an irony to me because you have often alluded to the European professors as having a certain distance from their students and kind of an aloofness. You point out that Wiegner had been quite exceptional in the extent to which he was personable, yet there still was a certain distance. From the impression you give me, he didn't pick up the shovel. By contrast, here in the United States, the students have a more familiar relationship with the professors and yet over there you said that the students have a more critical view of the professors than here. Was that a reaction to the distance that was set up between them?

Jenny: As to all of these things we discussed about Wiegner, I would think he had no idea that this was going on.

Wiegner's being more open than the standard European professor had to do with the fact that he would listen to our private problems whereas an ordinary professor wouldn't do that. Here that is a common thing. A student advisor knows the problems of the students, at least as to what courses to take and some other problems they may have. There is much more community here while over there, that didn't exist at all.

Maher: He was a good listener?

Jenny: Yes, he listened; he was interested in people.

Maher: How did he make you feel about yourself?

Jenny: Well, I would say positive by the way he invited me to collaborate in writing a book or giving me assignments and all that, and accepting what I did. Yes, it was positive--most of the time.

Assistants and the Fair Pay Issue, 1925

Maher: What were some of the other times?

Jenny: [pause] Well, when I was an assistant, the assistants had a hard time financially and I was elected as president of an association of assistants to improve the financial standing of our group. I did politicking on the national level and he wasn't keen about that. Probably he was right saying I shouldn't take time off and that it's a good thing for us not to have much money, but I felt he was too much for the establishment code.

Maher: Was that the point at which he made remarks about how a scientist should not be concerned with money?

Jenny: Well, perhaps that point was made during that period, but he made these remarks all along. He argued that you should be poor, or have a certain amount of poverty to be a good scientist and he emphasized the ideal aspect of science was searching for truth.

However he did not practice what he preached and some rumored that he remained in Zurich after the war because he liked the Swiss franc which ranked high even then.

He was, I would say, strictly an establishment fellow.

Maher: So you saw a reasonableness to his objections, but you had a feeling that there was an emotional element to his reaction to your working on behalf of the assistants trying to organize a betterment--

Jenny: Yes, he didn't like that I was taking time off and foremost, that I had certain ideas for change. The idea we had and which I expressed and formulated was that assistants are very important to professors, that a professor couldn't manage the whole thing if he didn't have us; and, of course, Wiegner wouldn't like that.

Maher: [laughs] The cat was out of the bag, it would seem. How did he confront you about it?

Jenny: Well, I would tell him I had to take half a day off or a day off to go to a meeting. So I discussed it with him, you see. Also, I was trying to collect signatures from professors to submit to the legislature and I had a hard time. I finally got one from him after I had gotten a list of other professors.

Maher: You finally got one from him?

Jenny: Yes, after a very famous, important professor signed first. I think I mentioned him--Laur--he was a great politician.

Maher: Before we leave that, this is new to me--about the organization of the assistants. Could you describe how that started?

Jenny: Well, there were economic problems, perhaps even a small recession in Switzerland. So they had to cut funds, and, of course, they started to cut down at lower levels and not on top; the old story.

Maher: What year was that?

Jenny: Well, that must have been in the mid-twenties, 1925, and so they reduced our salaries.

Maher: From what to what?

Jenny: It wouldn't mean anything in terms of figures nowadays, but sufficient to make it uncomfortable.

Maher: Like twenty percent or something like that?

Jenny: I would say at least a third. It was substantial, so we organized. I knew Professor Laur who taught farm management; I talked to him. He was a keen agricultural politician. But then I also went, without telling him, to the representative, you might say the congressman of the Social Democratic Party, and told him about our plight. So when the thing came up in the parliament, it just turned out that the most reactionary party, the Farmer Party, and the most leftist party, the Social Democratic Party, were both for the assistants. We got the cut restored retroactively so we got several hundred dollars each. My friend and I took a trip to Italy to have a look at the famous Sicilian red earth! [laughter]

Maher: I was wondering when you took that trip to Italy! How long a period did that take, all of that beginning to react to the cut, organizing, getting the guts to go through the legislature.

Jenny: Oh, I would say at least half a year. It took quite a bit of traveling to the capital back and forth. It was an interesting experience. Well, I said half a year. I didn't spend that much time on it, of course. But that's the period it took.

Maher: Was it mostly fellow soil or agricultural students?

Jenny: No. It was all over the university.

Maher: How large were the meetings of people?

Jenny: I would say fifty people showed up.

Maher: Out of maybe two hundred assistants?

Jenny: Yes. There was more interest than shown by attendance. Much was done by grapevine.

Maher: You couldn't be putting up posters or anything like that?

Jenny: Well, yes, I think we did.

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Maher: This is new to me. I wasn't even aware of it. How did it come about and--how did you find yourself in leadership. Do you think that you were amongst the strongest reacting to the problem or were you sort of drafted, or what?

Jenny: [pause] Well, I'm not sure. One thing is maybe I had some ideas about what to do. I guess that's all, that I had some suggestions, namely seeing these politicians and how to go about it. So there was that important professor, Laur, but the assistants also felt Wiegner would be on our side and he, in a way, was, but when he was asked to put down his name in black and white, he hesitated.

Maher: Do you think that revealed some lack of confidence that he had in respect to his colleagues?

Jenny: Why he didn't want to? Well, thinking back now I would say after all he was a foreigner in Switzerland, and I could see now that he would hesitate to participate or be active in this thing which he might really think wasn't the proper thing for him to do. That's the way I would explain it now, not that he was a coward or anything like that.

Exchange Reaction Research by Wiegner and Hans Jenny

Wiegner and Soil Science

Jenny: I never saw Wiegner holding a shovel, spade, or auger, much less digging a hole. Such work would not have been in academic tradition. Forty years later I experienced the same attitude in Germany. When I started to dig a profile in a forest, the assistants grabbed my shovel, shouting, "Herr Professor, not you, not you!"

To Wiegner soil was a disperse system to test his colloid chemical ideas. He was not interested in the soil as such.

Maher: Why don't we turn to the technical side of Wiegner's research. Maybe you can unravel the problems of base exchange that he confronted, and how he wended his way through those problems.

Jenny: I'll start out with a statement. In spite of the fact that we belittled him (and some of his personal habits), we were impressed by his thorough research. Work done for him had to be done very carefully. You couldn't engage in sloppiness. You had to have a scheme of what you wanted to do. Under the accepted procedures, you would discuss it with him and before you ran the experiment you threw ideas back and forth and weighed them and then performed the experiment. It was not the type of botanical experimentation that pokes a plant and observes its response. You had an idea as to what it might do. Then you would see whether or not you could elicit that answer from your object.

And what impressed me, for example, was that he had faculties that I didn't have. He was handling equations much better than I could because all the math I ever learned was really book math. He could apply it to real problems, which was entirely new to me; so this was impressive to me, that for a table with observational data you then would construct a curve and set up an equation. That was marvelous. These are things you can see. If I had an idea of running an experiment differently from what had been planned, I could see that he would either approve of it or show me that it was lacking in substance. So this was a rich experience for all of us.

Jenny: Now, I will talk about his ion exchange work in arable soils that he did in 1912 before he came to Zurich. He was then in Göttingen, I believe. He was an admirer of the old Justus von Liebig, who had denounced ion exchange, and he did a study. As a result of this study and as a result of other work he did in the colloid chemistry of milk, he was called to a professorship in Zurich. But that was before my time. So what I know about it is from casual discussions and seeing what he did. I don't know how he heard about base exchange, or ion exchange, and of Thomas Way's old experiment of 1850, by which Way percolated salt solutions through soil and analyzed them. I don't know where Wiegner picked that up.

But he could see right away that there was an unsolved problem, namely, was this a chemical reaction or was it a colloid chemical reaction, or a surface reaction? That's what he wanted to investigate.

He demonstrated in 1912 that ion exchange is stoichiometrical, ion equivalent for ion equivalent, a point that had been unclear these many years. Although this equivalency conformed to a chemical reaction, Wiegner found the law of mass action wanting and he preferred the idea of an adsorption reaction that followed Herbert Freundlich's isotherm.

He had to conclude that the ion exchange reaction was indeed a chemical reaction, but not a simple chemical reaction according to the law of mass action. He found, for one thing, that the reaction was not strictly reversible. You would not get the same constants, or parameters if you started from one side of the reaction or from the other. He realized it had to do with surfaces. It also depended on whether he had more clay or less clay in the reaction vessel. So it was a surface reaction, and to him, the chemical reaction and the surface reaction were not considered compatible at that time.

Problems with the Early Work on Ion Exchange

Jenny: Now, one problem, of course, was that the chemical reactions in those years were done mostly either with gases or with dissolved substances in solutions and there you had the so-called law of mass action satisfied. But also, the idea of activities in solution, or ion activities, was unknown then. Later that concept was developed here by Gilbert Newton Lewis on this campus. At that time the law of mass action, that is, the way it was formulated as a function of concentrations, wasn't always valid. It was least valid with these colloidal substances like clays, humus and proteins, which are solid bodies and have complicated surface configurations. So that was considered the area of surface chemistry. Now, since that time, you might say that pure chemistry has taken over the field of surface chemistry. It's not set apart anymore. It's just a more complicated chemistry.

Another aspect was that Wiegner had found great differences in the ionic exchange reactions between potassium and sodium ions, which in pure chemistry are not that large. The explanation that had come up already was that it had to do with the nature of the ion, namely the hydration of the ion. The idea was that ions are surrounded by shells of water molecules and thus have different sizes and much greater differences in sizes than non-hydrated ions. These sizes were not important in dilute solutions but on the surface of colloids this would make a big difference.

Maher: Who came up with that idea?

Jenny: [pause] Well, it appeared in pure chemistry in various areas, such as in conductivity and in diffusion measurements. The investigators came up with that idea. So, for my thesis, Wiegner encouraged me to look into that and establish the significance of the hydration of ions on these exchange reactions on soil colloids.

Research on Ion Exchange with Wiegner

Jenny: As an assistant in the mid-1920s, I resumed his previous work on ion exchange as a doctoral thesis, with emphasis on individual behavior of ions, the so-called lyotropic series that is attributed to ion hydration. I worked on that for several years, but Wiegner's ideas were extremely vague.

Maher: Did Wiegner show much puzzlement about what was going on? What can you tell me about his first investigations in base exchange where the chemistry didn't seem to apply in the old sense. At that time

Maher: he had not yet accepted the concept of activity used by G.N. Lewis to make the mass action equations come out exactly right. How did he deal with that?

Jenny: Well, that just came up then, at my time, so I had to look into the activities. While the activities could explain many things, they could not explain the big difference in behavior of potassium and sodium, because the activity coefficients are not that different in dilute solutions. Our emphasis always was that the reactions are not in solution; the exchange reactions are on the surface of colloidal particles, and that idea was actually correct. All work since has shown that, even more than we had imagined, because reactions take place in the interior of particles as happens in montmorillonite. So I think I came up with the idea then, as a sort of compromise, that inside the particle in the small pores, the ions are really dehydrated and behave differently than the ions which are on the outer surface, which are partially hydrated or fully hydrated and so, therefore, behave quite differently. So these so-called lyotropic series could take on all sorts of forms.

Wiegner then, with another student, explored the dehydration idea still further by running ion exchange experiments in alcohol where the ions are dehydrated, and indeed the ions behaved differently again. So this had a lot of interesting perspectives which Wiegner picked up and developed further. He put it into the concept of the electric double layer and showed this thing should have implications in other colloid chemical reactions like flocculation and stability of systems, and he put other students on that. So you could say he was like a general who had various fronts on which he operated and which also fertilized our own thinking.

So by the time I had finished my ion exchange work, I could see all sorts of new perspectives where this reaction would apply. One thing that interested me very much was what these reactions would do to the plant root. But I had no way of doing any experimental work along that line until I got to Missouri.

Maher: So he was able to widen your approach.

Jenny: Oh, it clarified what I did--that was the beauty of it. You see--everytime you produced a new figure it already meant something. So you were close to being an important person. Everybody in the lab was waiting for what you came up with and I was waiting for what they found. So doing research was an exciting cross-fertilization

Jenny: experience. It was not as if you were downstairs in your little lab all by yourself and beginning to worry, "What does it all mean?", as often happens. No, it was interesting.

Maher: Was it as if he was integrating the small scale efforts of many people and giving direction? Was he tying it all together so it had an overall perspective?

Jenny: Yes, he was the key person.

The Work of Wiegner's Assistants

Jenny: Now, what is interesting to me in looking back now was that, in such an environment, anyone could write a good thesis and do a good piece of research. Then twenty years later I looked at what had happened to all of these people who had worked with Wiegner. Well, as to many of them, I found that nothing happened because, as I look back now, I think they didn't see any further. They either couldn't get out of his way of thinking, or they took up other activities.

Maher: Was it because of his personal hold on them?

Jenny: No, it was that they adopted his scientific thought. He would say they weren't creative themselves. But I don't see it quite that way. I would say it takes other things. In my case I would say I escaped from him. Part of it was because I came to the United States as did C.E. Marshall, my successor at Missouri. I think that it was a complete immersion in a new environment that makes you challenge everything that you had learned. You were forced to challenge it.

Maher: And you, in turn, were challenged?

Jenny: Yes, that's right, yes. I think the people who remained there were all very excellent people, but they couldn't get out of that narrow conceptual sphere that he passed on to them.

Significance of Wiegner's German Background, His Changing Viewpoint

Maher: Do you think Wiegner's own transfer from Germany to Switzerland had had a similar effect on him?

Jenny: I would say that his transfer from a pure chemist to an agricultural chemist had an effect. Yes, I would think the move to Switzerland too, because his basic philosophy was challenged every day by the Swiss, who didn't have a Kaiser and all that. There was a difference. You see, he grew up with an emperor and the Swiss always have been anti-German politically, and it was very upsetting to him.

Maher: What was upsetting to him?

Jenny: Well, the Swiss criticism of the German political scene, and I'm sure it made him think of a lot of things.

Maher: Did he defend himself much, defend the German view?

Jenny: Yes, he did, but I would say not in a stubborn way. Of course, later on, many, many years later, when I was a Fulbright lecturer in Germany, I realized that my Swiss ideas of the Germans were not fully objective. Of course, he knew that right away, but it's very difficult to cope with it. He must have been in a difficult position. He couldn't always tell the people who made the remarks that they were wrong. That would have been impossible.

Maher: Do you think that the extent to which he was able to bring, according to your writings, new perspectives to whatever was being discussed was in part attributable to the fact that he was from another country as opposed to his own ability to see and discover a new view?

Jenny: Well, it did play a role. In Zurich, he had graduate students from French and Italian parts of Switzerland. They had the right to write their theses in their own language. Since he had to read those theses he had to learn those languages or at least become somewhat fluent in them. It must have opened up a wider perspective of the world. Yes, I'm sure it did, as I think about it.

Maher: Did he ever remark about that?

Jenny: [pause] I'm not aware of it but he did not remain, say, the typical fossilized German. He changed and he responded to the stimuli, whereas I know his wife didn't.

Maher: Can you think of any particular examples of either political thinking or scientific thinking where you observed him make changes in his opinions over the years?

Jenny: Well, I would have to project myself back to those years. At this point I should bring in the name of Max Kleiber once more. Max Kleiber was several years older than I. Before the First World War, he and a friend (W. Flückiger) migrated to Canada to establish a homestead. When World War I broke out, they came back to

Jenny: Switzerland. They said they came back to defend their fatherland from the war that was surrounding it. So Kleiber went into the military service and became an artillery officer and one day he became a conscientious objector. He was the only one at that time. This caused a big commotion and he had to go to prison. After the war he entered the university, but nobody really wanted him and he couldn't find any place to do research. However, Wiegner was the complete opposite of the traditional stiff German professor. He was jovial and out-going and treated his collaborators as humans. Wiegner offered Max Kleiber (later a famed professor at U.C. Davis) a research position in his laboratory. Wiegner's acceptance of him was, in our eyes, a great thing to do because the establishment had been against Kleiber, and Wiegner, in doing this, really took a stand against the establishment. Wiegner had sized Kleiber up as a very intelligent and creative person, which he turned out to be.

So in that way, among the young people and you might say among the anti-establishment people, Wiegner had a certain standing. I don't know what his colleagues thought of him but I never did see any antagonism there. Well, I would say if he had remained the loyal German he was when he was in the trenches, he wouldn't have done that. So he must have already changed his ideas about Germany itself. And I feel certain he would not have been a Nazi. I talked with him in the thirties, in the middle thirties, when the Nazis were coming up, and he was opposed to them. So I would think he had become transformed, from a good German to a good European through all of these experiences.

Another thing that we appreciated was that he often went to evening lectures and took us with him. There were lectures of the chemical society, of the physical society, or other lectures, or there were short courses in math or other related subjects. We would all go together and discuss the topics. At noon during the summertime when the days were nice, he and two or three assistants would walk down to the lake and go swimming and lie in the sand for an hour or two and discuss all of these things. It was a relaxing period. It was a close association.

I think he listened to us, and when I told him about our philosophical club, he got something out of the current ideas. He would talk about a subject, such as Dadaism, which was familiar to us, but not to him. That way he learned. He wouldn't say much, but I'm sure he applauded some of the things that the younger generation was doing and why they were doing it. There was this whole air, you see, after the First World War, this hope of having a United States of Europe, of having a different sort of governmental arrangement in Europe. Everybody talked about that.

Maher: But again he was mainly listening.

Jenny: Well, on some points, like the concept of a United States of Europe, he would have some special ideas. I'm sure that the Treaty of Versailles must have disturbed him greatly. But he didn't say much about it because the Swiss were strongly anti-German at that time because Germany had invaded Belgium and might as well have invaded Switzerland instead. So he was in a difficult position.

Maybe that helped him concentrate on science. Maybe if he had stayed in Germany he would have become a politician or taken a stand on something. In Zurich, his range of activity was narrowed.

V MOVING AHEAD IN COLLOID CHEMISTRY IN THE ERA OF EMERGING ATOMIC THEORY, 1920 TO 1926

Graduate Research with Wiegner on Colloids, Emerging Atomic Theory, and Impact of the Ultramicroscope

Jenny: Our research work, thesis work, depended on Wiegner's broad ideas of what colloid science really meant and contributed. We were well aware of the fact, for example, that the atomic theory of matter, which had been in many aspects well developed, was still sort of a conceptual scheme, and was not easily proven directly. The atomic theory fit the facts. It could explain the reactions and proportions and all that, but it was not really proven.

At the turn of the century many leading scientists were still skeptical of the existence of atoms and molecules. Thermodynamics was intellectually more appealing and "energy" and changes in energy was the common denominator. For example, famed Wilhelm Ostwald, the "Father of Physical Chemistry," had the word "energy" engraved over the entrance of his home.

Wiegner had been a student of a noted physicist in Göttingen by the name of Richard Zsigmondy, who had developed the ultramicroscope. It was a modern version of a light microscope by which he could "see" or visualize individual particles. When you looked in that microscope, at a suspension of colloidal particles, you could see them as little specks of light dancing madly around. These were real particles, larger than atoms and ordinary molecules, but not yet visible themselves. You could see what they were doing, as little light spots moving around.

So it occurred to observers that these particles behaved as you would expect gas molecules to behave or as solution molecules--such as sugar molecules in water behave--dancing around. Attempts were made to predict the behavior of the colloidal particles in the ultramicroscope and see whether the specks of light reflected the expected movement of molecules.

Maher: In other words, were these particles that could be seen that were colloidal in size, being bounced around?

Jenny: They were being bounced around and they exhibited the so-called Brownian movement that had been observed by a Scottish botanist, Robert Brown, a long time ago under a microscope as having spores--tiny spores--that wiggle and move around continuously. However, Zsigmondy's ultramicroscope was constructed so that you could count the particles and even calculate their size. You could do a lot of things with it.

Colloid science furnished the crucial breakthrough that sealed the acceptance of molecular reality. Colloidal particles, or macromolecules in modern parlance, could be observed directly in this newly invented instrument and the corpuscules (or corpuscles) behaved exactly as molecules would.

Advancing Atomic and Molecular Research and Einstein's Work on the Brownian Movement Theory

Jenny: Then came the great contribution of Albert Einstein who's beautiful Theory of Brownian Movement of 1908 offered the quantitative link. He was able to interpret the measurements of these particles--how far they moved to the left, to the right. He was able to integrate the distances with diffusion coefficients and he calculated the Avogadro number (the number of particles in a given volume for a given amount). Einstein was able to connect the crucial parameters of the molecular theory with the behavior of the colloidal particles you saw. It was possible then to calculate the Avogadro number from observation with colloids. The fellow who did this actually with an experiment was a Frenchman, Jean Baptiste Perrin, in Paris. This discovery or this development of Brownian Movement really put the atomic theory--or the molecular theory--on a very sound basis, convincing physical chemists like Ostwald. Here was clear evidence of the way the molecules and the larger particles behave, revealing a continuity of molecules and atoms and colloidal particles that gave the overall view of the corpuscular system of nature. So that was a great breakthrough. Oddly, this phase of the history of science is largely bypassed in American texts.

Using New Equipment, Methods, and Theories in Wiegner's Laboratory

Jenny: In Wiegner's lab everybody there worked with this ultramicroscope and we had to learn how to count particles. It was fairly complicated and, to be effective, you had to know something about

Jenny: probability theory to apply statistics to all of these things. But there we were--counting the particles and putting electrodes under the microscope and seeing how particles moved, how fast they migrated, in a process known as electrophoresis. All of these things were tremendously exciting and put colloid chemistry in the forefront of scientific advances.

Maher: Was Wiegner really making you aware of the overall significance?

Jenny: We actually had the tools right there and Einstein had just left Zurich after he had done all that colloid work on these theories in Berne, so it was all connected. There were the instruments, there were the great scientists.

Ascendancy of Colloid Chemistry, Stifled Creativity, and Wiegner's Contribution

Jenny: Colloid chemistry became much involved with adsorption of gas molecules and ions on particle surfaces, with viscosity phenomena that again involved Einstein. Valuable theories of colloid optics were being developed. Colloid science was welcomed by the agricultural, industrial, and biological investigators because the classical studies of gases and dilute solutions at equilibrium offered little guidance for their tasks. Being a colloid chemist became a status symbol.

Wiegner offered a special course on colloid science for chemists and physicists and elaborated on the "world of neglected dimensions": the colloidal realm.

In retrospect, colloid science in Germany--but not in France or England--became trapped in conceptual, phenomenological classifications that stifled creative growth. Wiegner did not fully escape its repercussions, as I had to experience painfully myself in later years. I remember endless discussions on adsorption versus absorption, on molecules versus particles, on crystallinity versus amorphic nature, or law of mass action versus surface reactions, on Donnan equilibria versus electric double layers, et cetera, et cetera.

Since the beginning of systematic soil studies of the proportions of course and fine particles in a soil, the mechanical analyses, or particle analyses, have been obtained by elutriation. Grains were separated by streaming water having various velocities.

Jenny: In the trenches, during the war, Wiegner had conceived a sedimentation technique that recorded all particle sizes, not only sand, silt and clay. Wiegner's sedimentation apparatus enjoyed a moderate popularity but was eventually displaced by the more convenient pipette analysis.

In Wiegner's hands, his sedimentation tube led to an inquiry into the stability of colloidal clay suspensions and how adsorbed ions and dissolved salts affect it. With the Finnish scientist [P.] Tuorila and physicist [H.] Müller, he formulated flocculation (or coagulation) mechanisms and expanded the [M. von] Smoluchowski equations. These observations and ideas have not yet found a way into the American arsenal of soil science, partly because of the preoccupation of American soil scientists with clay mineralogy.

Einstein's Viscosity Theory

Jenny: There is another vignette I should bring out which usually is not in the textbooks. As I mentioned, Einstein also developed a viscosity theory for suspensions of colloidal particles, and he came up with a very simple equation that had only one parameter, one constant in it--how the viscosity of a solution changed with the number of colloidal particles in it.

The story goes that Perrin, in Paris, had colloidal particles he had prepared--organic colloidal particles--and of which he knew the size. He ran viscosity experiments to check Einstein's equation. He got the results and he wrote a letter to Einstein noting, "I verified your linear equation on viscosity, but not the constant in it."

I don't know whether Einstein wrote him back to wait or what, but Einstein recalculated his own work and he found that the constant was not nine as he had published; the constant was 2.5. He had made a mistake. So he wrote about that to Perrin, and Perrin wrote him back stating, "Yes, I found 2.5." Now, that is written somewhere in the French literature, I think. That's the story we always heard.

Maher: That's tremendous to be able to react so openly and to be able to say, "Oh, well, yes, these are the empirical findings. I have to go and revise my thinking."

Jenny: Yes, that's very good. Also, it was very appreciated that a great scientist like Einstein could have made a mistake.

Reaction of Wiegner and Other Colloid Chemists to New Evidence

Maher: This reminds me that I wanted to ask more about Wiegner. I want to ask about his standing as a scientist and as a person and how all of these things were integrated together. The impression that I get is that among the qualities he had was his tremendous ability to deal with concepts and to express them in an eloquent way, in a way that commanded attention. It almost seems as though he needed that attention. I may be wrong about that, so I really need to put it to you as a question, but this is my impression. It struck me when you mentioned things that came later in your own findings, in your own research, in your ideas as they departed from Wiegner. You keep making remarks like, "Wiegner wouldn't like that," or "He is probably turning over in his grave to be confronted with these investigations." Yet those were empirical facts that were produced. I'm just curious, was he really being a great scientist in reacting to new evidence in that kind of emotional way?

Jenny: I would say the new evidence is not always as clearly established as some people might think, but it is also like a new fashion. When I was in Zurich, I heard a lecture by Svante Arrhenius who had advanced the idea that salts are dissociated in solution, which was a great new thing. There were chemists who would never accept it, who fought it, because there were many cases where this theory didn't make sense. Eventually, this dissociation theory became so fashionable that the opposition, so to speak, was silenced and the direction of research moved in that new line. This goes on all the time.

This also happened with respect to evidence on the crystallinity of colloidal particles. Incidentally, evidence on the crystallinity for clays was discovered here at U.C. Berkeley. The idea that colloidal particles are noncrystalline was a pillar of colloid chemistry, a very important one. It means that the atoms of colloidal particles are not arranged in a lattice and not nicely oriented because otherwise optical behavior microscopy would show it. But it hadn't shown it.

In another example, Debye and Scherrer, who were then in Zurich, had developed a new x-ray method of analyzing colloidal particles. They came up with the idea that colloidal gold was crystalline. That was a most upsetting thing. We knew, however, that colloidal gold was really a simple chemical compound, just gold (Au)--whereas, in clays and proteins and so on, you have many complicated components, many different atoms involved, and there was no optical evidence of orientation and organization.

Analyzing Clay Particles for Crystallinity Under Wiegner

Jenny: Nevertheless, without telling Wiegner, I asked my friend Emil Ott, a physics assistant who worked with Scherrer, whether he would x-ray clay particles for me to see whether they might be crystalline. That would have been another big thing. However, Wiegner had a wrong idea of what a clay particle is. His model was an artificial aluminosilicate which we always used. You mixed silica and alumina together which, according to colloid chemical principles, made a gel. Then you dried it and ground it up. It was a coarse material which could be used for softening water and it had high ion exchange properties and hydration properties--so it acted like a clay. That was our model of a clay.

Maher: Was that permutite?

Jenny: Permutite was the commercial name. So I gave Ott that powder and he x-rayed it and it was noncrystalline. So then I told Wiegner what we found and he was very pleased. Because of Wiegner's misconception about clays, this incident put Wiegner and his entourage on a wrong track and for years they stubbornly refused to accept clay crystal structures. It hurt Zurich's reputation.

Had I gone out and got a natural clay and had it x-rayed, we would have found that the clay was crystalline. The negative x-ray photo confirmed Wiegner's belief so strongly that he was not willing to accept the x-ray evidence that came out from Berkeley. Wiegner thought that only the nucleus perhaps was crystalline (from unweathered particles) and that the surface was a colloidal, noncrystalline film.

Today, we know there are many soil colloids that are not crystalline: the mogolite and allophanes. So it was not a black and white affair as we thought at that time.

Maher: Did you ever think that it was too bad you hadn't tried natural clay?

Jenny: [pause] Well, it would have been nice if after all these years and years of work on colloidal clays in Zurich that this could have happened there, but that's not the way the world operates. It takes curious circumstances. For example, at Berkeley, in plant nutrition, [Walter H.] Dore was starting x-ray work with cellulose and one day Professor [Walter P.] Kelley from Riverside gave a lecture about clays and said it was all amorphous; then Dore asked, "Maybe they are crystalline. Give me some samples." So Kelley gave him samples. But Kelley worked with natural soils, not like we did

Jenny: with artificial ones and, bang, there it was! But the correct crystal interpretation of the photo had to wait. It was not montmorillonite that had been recognized by German investigators.

Maher: What do you think you would have done if you had used a natural clay? If it had been a little later in your life, maybe you would have brought in natural clay.

Jenny: Wiegner would have probably sized it up and made the most of it.

Maher: Do you think he would have had the flexibility to change his views?

Jenny: Oh, yes, yes, he would have. He would have gone into it and done a lot of work to convince himself as to what he had. At that time, very little appeared in literature.

Maher: Do you remember that earlier I asked you about Wiegner's model? I noted that he diagrammed the colloid as being surrounded by negative OH ions,and that the whole body then became the negatively charged particle that would be moving in and involved in the cation exchange phenomena.

The way that struck me was to react by asking why the negative ions are clustering about this core? I would probably immediately posit in my mind a positive charge to the core. But if I were back at that time, prior to the x-ray crystallography, I wouldn't be in any position to be suggesting that there was an imperfect matrix, a crystalline matrix that was not balanced in its charge. But still I have that impulse and I am trying to understand from an historical point of view, what the views of people prior to x-ray crystallography were of that particle and I am trying to understand what things were excluding from their minds the possibility that it was a crystal and what things were excluding from their minds the question, "What about a positive charge that would possibly be holding the negative ions to the core?".

When you and I spoke about it, and I asked you what sorts of forces you and Wiegner were positing as holding the OH ion to the core, you said that they were like the positive charges of silica and aluminum that were exposed with a polarization of the negative portion of the atom on one side and a positive portion exposed on the other side, on the outside surface. Do you recollect that?

Jenny: The key to understanding the origin of cation exchange capacity of some clays is not crystallinity versus amorphism, but Marshall's isomorphous replacement of Al^{+++} for Si^{++++} in tetrahedra and Mg^{++}, Fe^{++} for Al^{+++} in octahedra. It applies to micas, feldspars, many clays and also permutite.

Recognition as an Innovative Colloid Chemist

Jenny: During the mid-1920s, my main task was studying chemistry, even when I went briefly to Paris to attend lectures by Nobel Laureate Perrin. Weekends and vacations were spent in the high mountains. There I stumbled over a real find--a true podzol! I returned to Zurich immediately to inform Wiegner that his teachings were valid, that the expected climatic soil type actually existed in the Alps. He came to see it and was elated. Now, he could teach his ideas realistically; before he had never seen a Russian climatic soil type.

Collaboration with J. Braun-Blanquet

Jenny: Within a small circle I found I now had a reputation. I was seen as an ongoing soil colloid chemist who also knew soils in nature, a new combination. People wanted me as a collaborator. I finally lined up with one of the younger ecologists, Dr. J. Braun-Blanquet who was developing a new field that came to be known as Plant Sociology. We worked together in the Swiss National Park. His taxonomic knowledge of European flora was overwhelming and he was anxious to place his concepts of plant communities on a causal basis. The collaboration culminated in a book in 1926: Vegetation Development and Soil Formation. We stressed the association and progress of the two, standing on a calcareous substrate and ending on an acid, podzol-type soil.

I did my own soil analyses but, as I was a pedologic autodidact (self taught), my profile descriptions were not up the the standards already existing then in the United States. In the 1950s, when R. Tüxen praised the book as a classic, I replied laughingly that it was a Jugendsünde (an illigitimate product of youth).

A new experience for me was the need to learn statistics. This was before the days of R. Fisher and small-sample statistics. A rule of thumb taught us not to compare means derived from less than thirty samples each, a sound piece of advice but requiring a lot of digging.

In one of the sections of this soil-plant monograph, I drew a speculative curve relating humus to climate, relying on some of Hilgard's analyses. It was a premature embryo, but nevertheless,

Jenny: the concept of a climofunction was clearly stated. I cite the instance defensively because in later years Professor Jacob S. Joffe in New Jersey used to tell people that I got the idea from him.

By then, in 1925, I had become familiar with some of Hilgard's work but I never dreamed that a decade later I would sit in his chair in Berkeley, California and teach his course on soil formation.

VI MOVE TO THE UNITED STATES: THE NEW JERSEY COLLEGE OF AGRICULTURE, 1926 TO 1927

[Interview 4: December, 1979]##

A Positive Attitude and a Scholarship Offer

Maher: This appears to be the place to ask you to recount how you decided to come to the United States.

Jenny: Well, the United States had been in the forefront all during my high school and university days because of its involvement in the First World War. We were all excited that the Yanks had defeated the Germans and the Kaiser. The most important event was the presence of President Wilson at the Treaty of Versailles. His Fourteen Points offered a great hope to a better future for Europe. Later, when the United States Congress decided against joining the League of Nations it was felt as a great setback. Nevertheless, there were strong positive feelings at the time about the United States, and especially its projections and its declarations of equality and liberty and all of these concepts--that I now call myths. But they had a great impact on Europeans.

So when recognized professionals went to the United States, it became quite an event and a few business people, architects, and painters who visited the United States would even print on their calling cards that they had been there. So that gives an idea of how positively many aspects of the United States were viewed.

Maher: Can you state that again, including the perspective of the United States prior to the World War I? Had there been a shift to that view of the United States from a previous less positive viewpoint?

Jenny: I wouldn't know about the earlier period because I was too young, except that I remember the phrase: "America, the land of corruption." But Europe was still in the period of colonialism, and all the happenings in the Third World, that we view critically now, including the role of the United States, did not bother us

Jenny: then. So it was strictly Western European-North American white supremacy ideas. The Third World was viewed a resource place for raw materials, and only at the time that I was a student did Albert Schweitzer become one of the first to point out the problems faced by the blacks. He went to Africa as a medical missionary and he awakened us to the idea that there are human problems on the Black Continent. But that only had an effect on some of the intellectuals who attended his lectures.

A fellowship to America was envied as a glorious opportunity. Scientifically, however, not much was expected. Most held American science, including soil science, in low esteem.* Science was a European adventure; U.S.A. had but one or two coveted Nobel Laureates!

The opportunity came for me when Dean Mann from Cornell University, representing the Rockefeller Foundation's International Education Board came to the lab and asked me whether I would be interested in spending a year at an American university. Of course, I was overjoyed.

Maher: How did he happen to ask you about that?

Jenny: Well, the International Education Board made a survey of various universities, as to what young, promising people they had. How I was picked I don't know. But I assume they knew that Wiegner had a reputation and so Zurich was one of the places they visited. But the interesting thing was, I had thought about the United States in connection with my thesis work.

When I worked on ion exchange with clay particles, I became curious about the ion exchange with plant roots. We knew roots were colloidal systems and had electric charges. It led to the speculation that plant roots, being colloidal systems, might exhibit the same ionic behavior, an aspect I had discussed repeatedly with my close friend, Albert Frey-Wyssling,** an ongoing plant physiologist.

The plant roots adsorb ions, so I thought ion exchange processes at the root would be interesting to tackle. We discussed these things, and we knew that in California, [Dennis] Hoagland was a good man in plant nutrition. So my first reaction was to go to

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*Today, in the field of soil science, remnants of this attitude seem to linger on, as judged for example by the small number of European subscribers to the Journal of the Soil Science Society of America.

**Albert Frey was his name then; later his last name was Frey-Wyssling, adding the name of his wife, a Swiss custom.

Jenny: Hoagland and learn about ions and plants and ion uptake. But Dean [Albert R.] Mann smilingly said I should forget about it because that was too far away. He urged me to select a spot a lot nearer to the Atlantic Coast.

Maher: What do you think was the reason for that?

Jenny: Well, I had heard of another case. A fellow wanted to go to Australia, to a botanist by the name of Robertson, and they wouldn't do that. I guess they wanted to spread the money they had among as many people as possible, so the trip to California, timewise and moneywise, was not to be encouraged. But I don't know if those were the real reasons. Fate arranged it that a decade later I did join Hoagland's staff, as an associate professor.

Maher: Another question I have is that I'm curious how it is that you and Albert Frey-Wyssling had known of Hoagland even though there wasn't much known in Europe about American science in the fields of plants and soils.

Jenny: I believe it was in connection with the beginning of international soil science meetings held in Zurich. I think that's where I heard about Hoagland. I don't think I read his papers but it was more by word of mouth. So, California occurred to me when Dean Mann spoke to me.

Maher: What were you hearing about Hoagland? About his ideas or about his skill as a scientist?

Jenny: I've forgotten now whether he was present, but I heard that he was working on ion uptake and plant nutrition. In Europe, these aspects were not treated extensively. During the war, the Americans had a big project on plant nutrition at various universities, hoping to devise (it sounds crazy now) a fertilizer formula that would give the best yields without regard to soils. These people were not soils people and I think Hoagland was in the picture sort of supporting that. The guy I went to work with in New Jersey--[John W.] Shive--he was a big proponent of this plan of discovering the optimal, ideal fertilizer formula to be applied to all soils. So that's all I recall right now about how this probably came about. Also, I might have heard about it from Frey-Wyssling.

So nothing was decided when Professor Mann talked to me. I think he was the dean of agriculture at Cornell. He questioned me in English trying to find out how much I knew, but I don't think I made a great impression.

In the mid-1920s two American postdoctoral students worked in our lab, Robert V. Allison and Marlin R. Barnette. Communication proved difficult. I had English in high school and we read Milton's Paradise Lost, but talking English was something else again.

S.A. Waksman's Invitation for Collaborative Study of Humus Colloids

Jenny: I guess I was selected on the basis of motivation rather than linguistic achievements.

Not much later, Professor [Selman A.] Waksman from New Brunswick, New Jersey, showed up. He had just written a paper in Soil Science with the title, "What is Humus?" He had the idea that there were microbiological implications in the formation of humus and he criticized the work of Sven Odén in Sweden, who had written a book on humus and who had a purely colloid chemical viewpoint. Odén extracted humus from the soil with alkali, not considering at all how the humus was formed in the first place, which Waksman was interested in. Waksman was innovative and pleasantly aggressive and he made a strong case that I should come to him and study colloid chemistry of humus.

Maher: How did Waksman come in contact with you?

Jenny: Waksman knew of Wiegner's work. Waksman was an internationally aware fellow, so he knew Wiegner's work. Over there, you know, there is much more contact by visiting scientist scholars. We had visitors almost every day from some European laboratory. Here, you meet at meetings. Over there, you meet on a weekend or on vacation, you go and visit other laboratories and that way we had the contacts. When he contacted me, Waksman was on a tour in Europe. Maybe he went to Russia and stopped off in Zurich. That's the way my destination was decided.

Maher: Waksman had earlier seen Wiegner at the International Conference on Soil, I think.

Jenny: There was a preliminary conference in Rome and maybe Waksman was there. Somewhere I have a photograph of the people who were at the earlier Zurich conference. I should have looked that up as to who was there, but I didn't realize you would probe about that.

Maher: Do you mean that when Waksman came to visit, you took a picture that you still have?

Jenny: Well, there was this first, preliminary, international conference after the war and a number of people came to Zurich. I remember a photograph. Maybe I still have it someplace. Maybe if I think about it, it will come back to me how Waksman happened to come to Zurich. Well, one thing was, looking backwards now, at the turn of the century, there was a very famous Russian microbiologist working in Zurich, [Sergei N.] Winogradsky. Waksman wrote about him and discussed his work and probably he wanted to see the room at the Technical Institute where his lab was, or something like that. It

Jenny: was sort of an emotional trip to see where his important work had been conducted. So I assume that was one of the motives Waksman had, because he wrote a book on Winogradsky and himself.

Maher: So when he was visiting Wiegner, did he speak with Wiegner and also with you?

Jenny: When he came we talked to him. The American opportunity was open to me to go and probably I asked him, "Where should I go?" Waksman took the opportunity and said, "Come to me!" [laughter] So that's the way it came about.

Maher: Do you remember much of what the three of you talked about with respect to soil or humus or Wiegner's work?

Jenny: No, Zurich was not the setting where, say, we would go out together and have a beer, or eat some place, no. I was in the lab and Wiegner and Waksman came in and we talked in the lab and that was my contact. Then the fellowship started to roll. Applications were made and, of course, Waksman wrote a strong letter to the Rockefeller Foundation that I should come to him and all that, and so the thing got pushed and before I knew it the thing was moving. I remember I was all excited and hurried and had to rush to get a visa. So I went to the American consulate and bang, it was locked, it was closed because it was Labor Day! You know, the consulates always celebrate the host country's holidays and also the American holidays, so there's often an accumulation of holidays.

Voyage to America and First Impressions of New Jersey, American Soil Science, and Americans, 1926

Jenny: The trip to America was a great event because I went by ship (the only way possible) and I was paid to go first class. So I had to buy a tuxedo which was a new experience. I thought I would do a lot of practicing of English on the boat, but I hadn't realized that I would get seasick, and most of the time I was seasick. So that thing didn't work out. I had bought a little pocket dictionary in England. I remember I had made some acquaintances with some nice ladies who had visited Europe. The whole ship was full of Americans who had spent the summer in Europe. So I remember one day, just beyond New York, a lady asked me, "Are you anxious to see New York?" I said, "Anxious?" and I looked it up in my little dictionary. It said, " Anxious--to be afraid of." And I said, "No, not at all!" [laughs] She was disappointed, but that's what you get when you buy an English instead of an American dictionary.

Jenny: I remember there was one fellow on the ship, an M.D. from Louisville, Kentucky. He talked to me a lot. He had been in Switzerland and was greatly impressed. He could not understand that anyone would want to leave Switzerland and come to the United States. Well, later when I saw Louisville, Kentucky, I could see that perhaps from the scenic point of view it didn't have all the advantages. But that depends on how you learn to appreciate landscapes. That's another chapter.

Anyhow, we landed in New York and distant relatives were awaiting my arrival. They took me to their home in New Jersey and we had quite an emotional evening talking about their ancestors and mine and so we got to bed late. But actually I hadn't seen any landscape yet, only buildings. So early in the morning I got up and looked out of the window and saw a large backyard. The relatives lived in a nice home, and when I went into the garden, the big surprise was that I saw an oak tree. I had never occurred to me that there are oak trees in the United States. I had more an idea that the vegetation would be exotic. So there were oak trees, and the boulders. There were also moraines! I was so pleased. It made me feel like home, because I had grown up among moraines. So ever since, I felt I had good contact with the American landscape, and it's really interesting to me how the geological formations can have this effect of bridging a transition from one country to another in a positive way.

Maher: It made you feel at home all at once?

Jenny: Yes, yes, I felt very happy about it because I knew my way around. I knew what kinds of rocks there were and stones and all that. Only later, I learned that glaciation really had come that far south. Now, I have forgotten which glacial period it was.

Then, in a day or two, I moved to New Brunswick, New Jersey, and met Waksman there. He was very helpful and right away took me in and assigned me a laboratory place. I met people. But he also categorized the various groups, the people who were good and the people who were not so good; the people who were his friends or not his friends. In a way, that was a disadvantage, especially with J.S. Joffe, who was teaching soil formation. I had few contacts with Professor Joffe because he and Waksman, both Russian Jews, were great enemies. So when I associated with Joffe, Waksman didn't like it and vice versa. So I didn't get the full impact of Joffe.

Also, the first thing I did was to take English lessons. Although I could read English and write it a little bit, talking was quite an effort, and I especially had difficulty understanding Americans. What we had in high school were English teachers from England and that English sounded so different.

Jenny: There was a period where I slowly got the feel of it. One thing I used to do there, was to go in the afternoon or in the evening to the railroad station at New Brunswick and watch the huge trains coming in. The trains and the engines were so much bigger than what we had in Europe. They announced the trains as going to "Trenton, Pittsburgh, and points West." And this expression "points West" impressed me greatly! I could envision the Indians and the cowboys and all of that, and far away, California. These trains raced through the station with big noises and it was an impressive thing.

Maher: What were your other impressions in running into the Americans, Waksman, his colleagues and your co-workers?

Jenny: Well, the rumor was, that at the New Jersey Experiment Station the only staff people they had were either Jews or Vermonters--people from Vermont. Somehow they had an influx of Vermonters. In the boarding house, that was what they said.

My first months at New Brunswick confirmed the European notions. American students spoke neither French nor German, they chewed gum in classes, lab work appeared sloppy, and discussions about football and funny papers took the place of those on the philosophy of science or European affairs. One Saturday, a Swiss friend Rudolph Geering, joined me in taking a night train to Pittsburgh to visit a modern art exhibit. The story of sitting up two nights in succession on the train and looking at crazy paintings during the daylight hours evoked amusement in the labs.

Gradually, I got to know other facets of American life. In graduate seminars and lab discussions, students knew more and could do more things than I had anticipated, and as my English improved, ignorance of my part came to the fore.

A Preconceived Concept of American Science Dispelled

Maher: How did your impression of science in the United States compare with what you thought of it when you were in Europe?

Jenny: As I mentioned, the European impression of American science was not very high. Of course, looking back now, it was the result of ignorance. For example, it sounds incredible to say that in the courses in chemistry we never heard much of Willard Gibbs, although we used his phase rule. When talking about free energy, it was about Helmholtz's. Of course, [Irving] Langmuir was known. In Zurich, organic chemistry was the big thing and, at that time, it was not very strong in the U.S. Actually, before the First World War, the Germans had this whole chemistry field covered with patents. We were told that one reason Americans went into the war

Jenny: was to get the patents from Germany. Actually, the American chemical industry started to grow larger after the war when the patents had been discarded.

I made a trip to Cornell where I visited Wilder Bancroft. When he heard that I was a colloid chemist from Zurich, he took me out to lunch and showed me everything. He was a colloid chemist and I had a good time.

The way we were influenced in Zurich, the city's scientific atmosphere, had much more to do with ideas than with detailed mechanical work. While in Zurich, the pH idea came up, and to get the electrical measurement of pH you had to build your own apparatus. We weren't very good at that; whereas in the United States, I saw people constructing all of the equipment themselves. So I could see Americans had much more ability with their hands in doing things and being involved with things, electrical things and whatnot, which impressed me; perhaps not so much manual dexterity as the uninhibited do-it-yourself spirit! Over there, all of these things were done more or less by specialists--mechanics and glassblowers and all that. Actually, even at Missouri you could take a no-credit course in glassblowing, taught by Professor [William A.] Albrecht. So that was one thing that impressed me.

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Jenny: In many ways, my training was deficient or one-sided. I remember I ran once into a discussion of thermodynamics and oxidation/reduction potentials, and it became clear to me that I didn't know as much as the people I talked to within the Department of Physical Chemistry. So, I gradually learned that my ideas needed revision. Apparently it evolved gradually, but fairly fast, because during the next summer when we had the international congress in Washington, and I met the Swiss people and a lot of European people and they said, "Already you have become Americanized" because of my views. So I must have changed quite rapidly.

Maher: So by and large you were more impressed with American science than you had expected.

Jenny: Yes, that's right. I soon learned that. Of course, the big new thing for me was the soil survey activities. That was really an eye-opener. Americans had the sort of detailed soil maps in colors, accompanied by accurate profile descriptions, that I had dreamed about in my alpine work. Here, they existed on a grand scale. (Later I learned that a beautiful map is not necessarily an accurate one.)

American Soil Analysis and Mapping

Maher: What was the reason for the big difference between soil mapping in the United States and its absence in Europe?

Jenny: Soil science in the United States got started very early. Later, when I did work on [Eugene] Hilgard I could understand his thinking. The reason was essentially that all of these farmer immigrants from Europe came to the United States and settled on new land. But actually they were confronted by new situations as to soils that they had not known at home, like lime horizon and sodium content. Everything was so different and there was a need for having soil maps for the immigrants and the settlers so they could get an idea of what they had. So that was started in the late 1890s by Milton Whitney and his group in Washington. They started the soil surveying technique. Before that, Hilgard had published soil maps of the southern states. But these were on a scale that was all right for geographic interests or county planning but not for individual farms. The scale was too small. Whitney--or one of his subordinates--introduced the idea of making soil maps that were large-scale enough so you could see your own farm on it and get to know your land.

This soil mapping element was completely missing in Europe. Where I grew up, the farms had been in the families for centuries and every parcel of land was known--its drainage conditions and all its features. Whereas, on a new continent, soils were unknown. People would settle on a homestead. It might turn out to be very poor land, as in parts of the Ozarks. Or you might have gotten very rich land as in Iowa. Then you prospered. So the need for information was pressing.

Maher: In the European agricultural training that you were exposed to, did you find much emphasis on the need to shift to another set of practices for a different soil. In other words, was there much recognition of the different types of soils?

Jenny: Yes, there was but in terms of slopes, in terms of texture (whether it was sands or clays), and in terms of water table (whether it was low or high) and fertilizer needs. These were the features. Really, it wasn't too wrong because Western Europe is all the same high rainfall climate. Further, Europeans had detailed geologic maps. Large soil differences did not exist as they would in the huge continent that includes the United States. Later, when I wrote on Hilgard and studied the history of soil science in the United States and the role of soil in the development of the continent, I began to realize that not only did the European farmers who came to the United States bring along their own prejudices about what is a good soil, they also didn't know what a good soil is in a given environment. In Western Europe, a good soil was a soil that was a

Jenny: clay loam, perhaps for grassland, because that was the big thing--dairy farming--so you had to have good pastures. So a good soil had a fairly heavy texture.

Then the settlers came West and encountered the sandy loams; they thought they were no good, but these were the best soils! I heard in Missouri from Professor [Henry H.] Krusekopf that when his family came from Central Europe, they would avoid the prairies, because the idea was that if the soil cannot support a forest, it cannot be very good. So they started farming the poorer land that was timberland and the good grassland stayed virgin until a later period. Besides, they couldn't plow the prairie easily because of the dense sod.

Maher: Did you ever actually meet Milton Whitney?

Jenny: Yes. I saw Milton Whitney. But his name didn't mean anything to me at that time.

Maher: Was that during the first year?

Jenny: Yes, I think he was in congress. I'm not sure now. I got to know him personally through Hilgard because he was his great enemy, though that's a later story.

Maher: Okay, what's puzzling me is that you were coming over to Waksman's laboratory and he was pretty much a soil mircobiologist and I'm curious how you came to be exposed to this aspect of the soil mapping in U.S. soil science.

Jenny: Well, that started in Switzerland. While I was studying colloid chemistry and working in the labs during the week, and on weekends, Saturdays and Sundays, I went to the mountains with Frey-Wyssling and others. I was interested in soils and tried to find climatic soil types found in Russia. Also, I had taken a lot of geology so I was used to being in the field and digging and all that. So naturally I grew into the field aspect and further; before that, I had spent a year farming. I was interested in the broader and applied aspects. On my summer vacations, I worked with Braun-Blanquet in the Swiss National Park doing studies on the evolution of soil and vegetation. So I had a good background of interest in field work.

We had seminars at New Jersey and the soil surveyors came and talked about these things. Of course, I talked to them and they took me out to the field and it was impressive.

Maher: Was Joffe, or would he have been, much of a connection toward this kind of study?

Jenny: Yes, but Joffe wasn't on the best terms with the soil surveyors either.

Maher: Could you explain what you knew about that?

Jenny: Well, the only thing I know was he never came along when I went out with them on inspection trips. But I wasn't especially interested in trying to find out why.

The person who impressed me greatly was Dean [Jacob G.] Lipman. He was dean of New Jersey's College of Agriculture and, incidentally, he was the brother of the [Charles B.] Lipman here, whom I got to know very well.

New Jersey was a state university. It was downtown with old buildings. They always had financial problems and when the College of Agriculture expanded under Lipman, the story was told that first he bought up lands on the outskirts of New Brunswick. Then he worked on the legislature to establish a campus there.

They tell the same story about Davis, California, and what the professors did at Davis--the same thing--they bought surrounding land and then they lobbied to get a main campus. I don't know how true all this is, but it was considered a smart move, so I guess it's sanctioned to do that.

Research in S. A. Waksman's Laboratory and Transfer to Professor John Shive's Greenhouse for Ion Studies

Maher: Well, now take us to Waksman's lab and your real workaday situation.

Jenny: Okay; instead of using the techniques of Sven Odén of Sweden of simply extracting soil with solutions and getting the black humus substances out, Waksman had me to start with leaves. So we got oak leaves and other leaves and put them in pots and let them ferment and decompose. That would be done for weeks and months, and the idea was that every few weeks or months, we would analyze what there is. It was a sound approach, but I couldn't visualize when I was going to get those humus substances, and how to study their colloidal properties. Then also, the lab was in an old basement which, as noted earlier, was leaking. During rains it was all flooded and you had to wear rubber boots to work there. The people who were there--the students who worked there--I don't know whether they were graduate students or not--I felt that they didn't even know how to measure a certain solution of sulphuric acid; it was, to me, very sloppy.

Jenny: For example, not considering the density and anything like that but just pouring it together. I was horrified, but of course I didn't know that for these experiments that wasn't important. It's just as if you send a watchmaker into a foundry with different styles and different approaches.

Maher: Different levels of resolution.

Jenny: Yes.

While Wiegner's physiochemical lab was spick and span with ultra clean benches and mirror-like floors, Waksman's biological lab, in the basement of an old building, was messy and required wearing galoshes whenever it rained. So I then quit. I tired of it and switched to Professor Shive's greenhouse, working on effects of ions on transpiration by barley plants.

Years later I realized that lab-life style's are not crucial, for it was Waksman who got a Nobel prize, not Wiegner. My transfer to Shive must have hurt Waksman but he never let on and kept me in his circle. He was magnanimous and we remained friendly to his end.

I had taken a course with Shive on plant nutrition and he was quite impressed by the ideas I had about ions entering roots, and exchange capacities of roots. So I ran an experiment in his lab, in the greenhouse, which never was published but had really striking results. We grew barley plants in solutions of various concentrations of calcium chloride, or magnesium chloride, or potassium chloride and, for a day or two, measured the water consumption, the transpiration, and there were striking differences, showing that the solution ions affected the water uptake of the root, in a characteristic way.

Maher: Was there a pattern that you were able to discern?

Jenny: Yes, I had what is called a lyotropic series. But I had some, well a few, experimental points that were lost because of breakage of the pots, and so it wasn't a neat thing that could be published. It should have been repeated, but I didn't get around to that.

Maher: I guess that was one of the disadvantages of just being there for a year.

Jenny: Well, less than a year. It took me several months until I had completely switched over. But all during that time I was in Waksman's circle and he never let on. He was very kind. At least, even to his death, he would always invite me and was on my side and helped me out. He was a great guy. I was profoundly impressed by him and by Lipman who was very broadminded and generous. It was a good place.

The Dual Scientific Approach: Lab and Field Work

Maher: You had come to Waksman's lab with basically two backgrounds or at least two big categories of work, it seems to me. One was the very strict, chemistry based investigation of the soil as a colloid system, and the other was based on your work in the moraines in the Alps looking at the larger picture, wasn't that approaching soil from opposite ends?

Jenny: But to respond to that dualism, you see, I would have to be in different departments. Waksman was not concerned, or his students were not concerned, with outdoor soils work, and the soil surveyors were not concerned then with the laboratory work, although the gap had been bridged in the Middle West. Here in California, because of the climate, the soil surveyors worked in the field all year around. In summertime, they worked north, in wintertime down south. They were always in the field, seldom in the lab. In the Middle West, you worked in the field in the summer; in wintertime the land was snow covered so you spent many months in the laboratory. Surveyors became quite involved with laboratory work. That was at Missouri. Definitely [Richard] Bradfield was a great laboratory man and was very good and he would also have outdoor agronomic interests, but we'll take that up later. It would be interesting to view that thing, how it is, for example, in the South, in Louisiana and Georgia, where perhaps you could work in the field all year around. Especially when the pressure for soil survey work is intense, then the lab work suffers.

Maher: So when you got to Waksman's lab and you were working with him, it was pretty much a matter that you would be working only on the more rigid and narrowed-down approach rather than the broad spectrum.

Jenny: The idea was for the colloid chemical approach, yes. But the colloid chemical approach is really very broad. You see, at that time, what we learned about the colloid chemical principles was that, yes, they applied to clays, they applied to humus, they applied to bacteria. It was a broad thing. So I could always participate in discussions over a broad range and that is still so because it's not a type of chemistry that is just true for dilute solutions or ideal systems. It is a thing you encounter in factories and in the field.

Maher: It's a step toward the complexity of soil or other systems, but it's not such a big step that you abandon all of the rigor of chemistry or physics.

Colloid Research in America

Jenny: The scientific developments have been different. For example, in this country, colloid chemistry never had really a strong foothold. There was Wilder Bancroft at Cornell. There was a good school in Minnesota with [Ross A.] Gortner. There were others. But they were specialized--in biochemistry or in microbiology--and the broader colloid science field was never really developed. I said Minnesota--but also in Texas at the Rice Institute, Wiser had a colloid chemical group. They specialized studying oxides of heavy metals. There was the approach of Langmuir, who was really dealing with surface chemistry. His elegant experiments of the orientation of molecules on surfaces and on liquid surfaces developed into a phase of colloid chemistry, named surface chemistry. It became a big thing in the United States. But workers would not discuss, say, flocculation or other aspects of colloid chemistry.

More on Waksman: Research Scientist, Colleague, and Friend

Maher: Do you think Waksman appreciated the potential that colloid science had and its breadth?

Jenny: No, he was really opposed. But I'm not criticizing Waksman. I agree that his approach to my research project was very sound. But it would take several years' duration to get these colloid chemical substances. Then he also wanted, of course, to have all the bacteria identified and the fungi identified and actinomycetes. Well, that was all great. I never got to what I really had wanted to do, because he was opposed to going out in the field and to taking the soil and extracting humus. He was right in a way. I had bacteriology in Zurich. But there, it was merely applying methods of nitrifying bacteria or of isolating bacteria that would create acids. There was not much about processes. It was more about the taxonomy of bacteria. So, in New Brunswick, I learned a lot, an enormous amount, with Waksman and his group and especially his students, Robert L. Starkey, René Dubos and others. They were deeply involved and I, just by osmosis, sucked it all in, all they had to say, and I'm sure my interest in microbiology is related to that period. Waksman, at that time, had just written his thick book on soil microbiology and it was a stimulating document.

Maher: Was it a new realm for you to be considering, as part of the soil microbiology, not just the bacteria but the actinomycetes and the fungi? It seems to me there was a shift at that period. Was there an opening up to realize these other realms existed?

Jenny: Yes. He stressed the idea of microbiology, including the fungi. But he did not, for example, stress invertebrates. He was a microbiologist. In fact, this came out at the meeting at the Vatican in 1968, when we were both on the same program. I reported on functions, but he was not happy with my systems approach. He asked, "What bacteria were involved?" Well, this fellow at Cornell, Professor Alexander, who attended the Vatican meeting said to me, "Well, Waksman wants microbiology. He can't understand your views of systems analysis." So Waksman never changed. He was a microbiologist, a reductionist. He stressed fungi. They were involved and they had been overlooked, and I'm sure he was right.

Maher: Now, going back to the time when you were first in the lab with him in 1926 and trying to decide how you were going to fit in with things. Did you have the feeling that he didn't appreciate getting too broad and that your interest in trying to identify a whole system was looked upon by him as being unscientific or not worth the trouble?

Jenny: Well, at that time I had not yet developed a broad systems view. I never had the feeling that he was narrow. He talked about so many different things. He never showed up in the lab or at the midnight snacks. He was, what you call a promoter, who also had depth in analyzing problems in his studies.

Maher: Did he always have his own project going--one that he would devote himself thoroughly to, or that got a portion of his time?

Jenny: I can't say that. You see, as always you had to have money, you had to get money. He got quite a bit of money from sewage districts for work on sewage. He had several students on that thing. So his interest also had to do with getting funds to support the students who wanted to work with him.

Maher: What sort of projects would be done on sewage?

Jenny: Well, one was isolating the bacteria and the microbes, in general, measuring CO2, adding nitrogen to see what it would do. With all of these applied projects, you see, somebody wanted to sell something, either a fertilizer or a chemical or whatnot.

Maher: Was that really pioneering work?

Jenny: I don't know. I didn't follow that line of research.

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Jenny: Once, when Waksman went to an ACS meeting [The American Chemical Society], he invited six or us to come along. To help us out financially, he rented a large hotel room, and then at night we sneaked in and slept on the rugs and in a big drawer. [laughter]

Jenny: He was very free with opinions about other people, other workers. He would say, "This fellow is good. This fellow isn't good." He had us all categorized, which was a valuable thing to do. But, of course, it was a form of bias toward individuals. If he denounced someone, we couldn't help feeling negative about the guy unless we knew more about him. I know this caused a lot of friction.

Maher: How did it make you feel about yourself or others who were new at his lab? Did you fear that you would be categorized similarly?

Jenny: No, I felt that he was always positive toward me. I guess, as a former student of Wiegner's, or more because I did some funny things that amused him, I think he liked me. Also, I gave a few interesting seminars. For example, in Zurich, we had a student who tried to correlate the European soil types with rainfall and evapotranspiration. Alfred Meyer was his name. He developed a sort of a scheme, a NS- quotient, that permitted correlating climatic moisture values with soils. So, while working under Waksman, I took that over and very boldly constructed a climatic moisture map of the United States saying, "There is no law against doing it." I predicted, for example, that this place (e.g. Kansas) you must have chernozem. You must have a soil with a lime horizon." Well, that impressed him. I didn't know if it was true. I said it must be so because I believed whole heartedly in this climatic dependency which was an oversimplification. But nevertheless, I brought these together and talked about humus, especially where much humus should be found according to climatic principles. Waksman was quite impressed by this. He always invited me home and he took an interest in my private life, too.

I remember some years ago (I've forgotten now where it was), that he and Mrs. Waksman and I talked together and that they asked about my daughter, Joanna. They knew her and they knew she had married a Jewish boy from New York. Mrs. Waksman asked me, "As her father, how do you feel about that, having a Jewish boy in your family?" I said, "He's making my daughter happy. What else do I want?" She said, "That is a good answer." But perhaps this gives you an idea of how close we were.

When Waksman retired, he thought perhaps he would come back to Berkeley. He had graduated from Berkeley. He came to my home here and stayed a day or two to survey the situation and to think about whether he should move to the West Coast. He finally decided against it.

Maher: Were there any other seminars that you gave?

Jenny: Yes, I gave seminars in chemistry classes on my work on ion exchange. Yes, it was a positive thing.

Jenny: Of course, I still had all of the negative views about people chewing gum [laughter] and not being interested in European affairs, which to me, seemed to be the most important thing in this world, instead of all of these Americana things. Because it was so strange to me. I enjoyed reading the books of American writers or artists who were somewhat against the establishment or against the Americana, such as H.L. Mencken, Sinclair Lewis, and Thorstein Veblen. I also felt that the spirit of Henry George was still alive. The scientists in Waksman's laboratory talked about these people. They also disapproved of what is called typically American.

Social Stratification: America and Europe Compared

Maher: You told me that when you were going through schooling in Zurich, you really noticed the stratification of social groups. Did you find that also in the United States?

Jenny: [pause] Well, what I found was surprising to me. I have to step ahead a few years to illustrate how long I was bothered by the need to be aware of the social class background of co-workers. One evening, when my wife at that time and I were in Columbia, Missouri, we didn't know what to do. So we decided we would go to a movie. Now, going to a movie in Europe was seen as an inferior thing to do. That was only for the lower classes. You go to the opera, to the theater and you go to symphony concerts and all that. You don't got to movies. But in Columbia, Missouri, that's all there was. People went to the movies. So we sort of sneaked in and I had my hat over my face so as not to be seen! [laughter] Then during the intermission when the lights went on, what did I see? Three rows in front of me was the dean of the College of Agriculture! Well, that just floored me that this would happen. Eventually, I saw all of the other deans there, too.

Maher: In Zurich, people who were scientists were perhaps not in the category of medical people or other professions, but you have often mentioned that European scientists are characterized (and particularly the professors) as being disdainful of "getting their hands dirty" and that sort of thing. Will you please tell me about that.

Jenny: Yes, that's right. Another thing that surprised me is unbelievable to me now. In New Brunswick, the college had a party. I don't know whether it was an open house, or a Christmas party, or what it was. But I went there and the dean was there and the president, and, to my surprise, the secretaries, and that was to me incomprehensible, that the secretaries would be there. Then I brought that up and one fellow, I still remember, warned me and said, "You have to be very careful with the secretaries because next year perhaps they might be

Jenny: the wife of your boss." Well, I couldn't imagine that somebody would want to marry a secretary. So I had all of these preconceived ideas.

Maher: Did it seem to you that there was a connection between that mixing and what you discovered in the laboratories that people in the labs were much more able to put together their own equipment and develop a means to carry on their experiments? Whereas, in Europe, you were saying that it would always necessitate a specialist being called in.

Jenny: I would think this had to do with two things. One is the car (the automobile), and I saw that with my own son. Kids take cars apart and they become experts. The other is the idea of do-it-yourself. That philosophy was not in Europe. Maybe it is there now, but you wouldn't do that because the job done had to be first class and you couldn't do it first class. But here it doesn't matter. It's much better if you do it yourself. Even if it isn't professional, that's considered better. Over there it was the opposite.

Maher: I'd like to hear about the other grad students that you met or the students who had come to Waksman's lab. You mentioned some--Starkey and René Dubos. Can you explain what they were like or what their work was or what the rudiments of their ideas were?

Jenny: Waksman drew a lot of students, and looking back now, some of them were not very good and I assume those were the ones I was with in the lab. Gradually they were eliminated, but he had all of these fellowships and he had to get people and so I would say he had a very broad spectrum of outstanding people at one end and poor people at the other. In that lab when I first started, I was in the wrong group.

The most outstanding fellow was Starkey. He was Waksman's right-hand man and he already had a good reputation in microbiology. I think he worked on iron and sulphur bacteria. Dubos came as a graduate student from France. We had many discussions together. He is a famous man now, retired too. He perhaps had the broadest philosophical viewpoints. We had a lot of contacts. At that time, I spoke French fluently, so we usually talked in French. But he impressed me very favorably. [J.H.] Blom, from Denmark, speculated about the first compound in photosynthesis.

There was a fellow, Fagundes, from Brazil. He was a pleasant fellow, a good talker, but he was not outstanding.

Maher: What was it about Rene Dubos that impressed you?

Jenny: I think, his broad views of microbiology, and cultural aspects. He had the regular training of the French students for the Baccalaureate exam to enter a university, which demands a very rigorous training. So he was skilled in Greek and Latin. Later that showed up. He had these broad ideas.

Looking back, I don't know now whether the idea of antibiotics was already cooking, or whether it came later and I heard about it with my later contacts. During the thirties, Waksman arranged symposia and meetings, and he often invited me to those. So I was with him again and learned new things. Now, I remember, he said in a lecture that it surprised him that the diseases of the Middle Ages, like the bubonic plague, vanished. That, if the dead body was buried in the soil, the disease disappeared, and the soil was no longer contaminated. You could dig it up or touch the soil and you wouldn't get the disease. He expressed then the idea that there may be substances produced by microbes that counteract those pathogenic bacteria. But I don't know for sure now, whether that was in 1927 or later, after Dubos. Dubos got on it because of his wife who died of T.B. Then he was working on that, so I don't know. I'm sure that's in Waksman's writings about the history of the whole thing.

Someone said that Waksman wrote an autobiography, and that I am mentioned, but I have never seen it, so I don't know.

Life was interesting and I had no concerns about the future. Today, it would interest me to re-read the letters I sent to Zurich in those days.

The First International Congress of Soil Science, 1927

Involvement in Preparations for the Congress

Maher: Let's move on to the International Soil Science Congress that was held in the United States in 1927, in June and July. That was still during the period that you were with Waksman, is that correct?

Jenny: Yes. When I was at New Brunswick, preparations were made for the congress. I remember that I and Blom, from Denmark, were asked to translate articles because all of the papers needed translations into several languages. So we either had to translate French and German into English or vice versa. I did a lot of that and so did Blom. We were quite malicious about it. We had fun in the translations, giving a certain rhythm as that of a hexameter or a pentameter. We would translate, making fun of pompous writing. So, in that way, I was involved months before the event really happened.

Maher: So you had a pretty good idea of all of the papers that were going to be presented?

Jenny: Well, quite a few, especially those that were main papers by the principal speakers.

Maher: What papers were those?

Jenny: There was one by Lipman, broad papers about--well, at that time perhaps not world hunger, but the importance of soil science to humanity and stuff like that. As these manuscripts came in, with phony allegations of the importance of soil science to saving humanity, we translated it in these styles to sort of make fun of it.

Wiegner came over with a delegation of Swiss people. He had been able to get me appointed an official Swiss delegate, which greatly eased the financial burden. Wiegner lectured on my Ph.D. thesis work on ion exchange and I reported on my Alpine soil analyses. I was thrilled to get to know the great soil scientists of the era in person. I've forgotten now where I stayed in Washington, D.C. It was hot. It was a great thing. It was the first International Congress of Soil Science. It was superbly organized by Lipman, and very modestly he stayed in the background and let other people push themselves.

I was still under the impression, along with other younger people, that the old fogeys in soil science didn't know much, and that we were the coming generation, and I assume that attitude still exists today. We have new generations waiting to take over. I found this attitude less noticeable in the United States. One reason it is different is that the professors here are more concerned with students, with doing a good job and lecturing and making contact. So there is more uniformity and give and take than over there where the difference between a professor and the students is enormous or was so. I understand it has changed, too.

Reaction of Wiegner and Waksman

Maher: What was your impression of the scientific caliber of the people who attended the congress? Was there all of the cream, so to speak, of soil science?

Jenny: I would say there were some very good workers. I would say I had underestimated the work of the older people and later I got to appreciate that more. There were famous names like C.D. Glinka, and Peter Treitz from Hungary and Teodor Saidel from Romania; a lot of good work was presented.

Jenny: Others prominent among the seniors were Sir John Russell of Rothamsted, D.J. Hissink of Holland, D.G. Vilensky of Russia, and the German, Albrecht Penck, father of the four glaciations. On Pikes Peak I pointed out an extra-beautiful Carlsbad twins (in granite) and Penck was so pleased to hear a mineralogical word that he had a photo taken of the two of us.

But both Waksman and Wiegner were rather negative, and that came out on the field trip. Wiegner had his colloid chemical theories and when other people tried to explain a profile differently, he denounced it to us and said, "This is stupid," and stuff like that. So I didn't have a really objective evaluation.

Maher: What do you think was Wiegner's and Waksman's analyses of the calibre of the people as far as scientific work was concerned? Were there objections because they had strong intellectual differences or did they arise from the feeling that these people were intellectually shoddy or second-rate scientists?

Jenny: No, I don't think it was the latter. I would say that they didn't know enough beyond their specialty. Wiegner knew nothing about soil physics, and this was the big topic. He had no idea of field capacity, or of permanent wilting percentages. This meant nothing to him. He had no realistic concept of the amount of water in the soil useful to plants and all that. What he didn't understand, he thought was no good.

Waksman also had problems. In Colorado we had a field discussion about nitrate accumulation in soil, by the local people from the Colorado Experiment Station. Waksman made fun of their explanation. But, I think, Waksman didn't know enough about soils in the field.

It would be worthwhile to analyze at a modern congress what is considered valuable and what not. How many would recognize a new feature that holds promise? Today there is emphasis on biochemistry and thermodynamics. A congress where these things are not in the foreground, soil chemists might say it's not worth it, and maybe it isn't to those people. Or, look at Professor Storie. He represents an age. He did very important work and still does important work with his practical soil classification, but he has no echo in the university because the young people think differently and use different language, and symbols. For him, there is a big gap in communication, except with one or two people, like Rod [Rodney J.] Arkley and Mike [Michael J.] Singer.

Maher: Can you think of any germinal ideas that you were exposed to at that first congress which were later very fruitful when pursued, but you just didn't realize it at the time?

Jenny: A lot of discussions were about mechanical analysis of soils--physical analysis of soils. That was the big thing then, and there were lengthy discussions. Wiegner was involved; the Swedes, with Sven Odén, were involved; Sir John Russell of Rothamsted, the Danes and the Russians were involved.

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Maher: Could we put it another way--that this was the soil physics end of things?

Jenny: Yes, the soil physics. Mechanical analyses was the soil physics in Europe, not the field of soil moisture. Since it rains a lot in Europe you had no problem of water deficiency.

Looking back now, I don't have a good recollection of what feat was accomplished at the congress. Part of it was perhaps that we knew this great continental excursion was coming and our interests were already centered on that.

Maher: Everybody wanted to get going?

Scientific Stimulation on the Continental Excursion

Jenny: Yes, of course. The attendance was limited. I don't know how much it cost. I think you had to be an official delegate to get on, and that the excursion was really a magnificent idea. I think Lipman was behind it. He did the organizing and it took an enormous amount of logistics to get all of this under one hat, to organize the stops and have the soil pits and profiles dug, and to have all the cars for the field trip read. We had this whole Pullman train and we lived there day and night. You were assigned to a specific Pullman car. It had a name and that's where you lived. You didn't have much space for books or soil samples.

I remember there were some frictions, too. I guess you have never seen the Pullman car. But you have lavatory rooms and wash basins, and there was always friction as to who kept the facilities clean or who had to clean up for the next guy. I suppose it is the same thing nowadays in the communes: who was leaving a mess and who was cleaning it up? It was a big thing then and it wasn't solved.

We always traveled the big distances at night. Then we had breakfast on the train. Then we had a stop and all of the local excursion cars were lined up in rows by the owners, mostly Model-T Fords. So we had a caravan going to the various sites of things to see.

Jenny: Invariably, and embarrassingly, many of us fell asleep during the afternoon sight-seeing tour because we had been arguing too long the night before.

Imperceptively, a generation gap arose. In front of soil profiles we youngsters of all nations kept talking about pH, the promising new concept, about exchangeable hydrogen ions, ion activities, and zeta potentials. The elders, on the other hand, asked about textures, total analyses and fertilizer responses. Different interest groups slowly evolved.

The story developed right away that when you arrived at a big soil pit, the Russians were in there first, and you practically had to throw them out for the other people to get in!

Maher: Were they perhaps the ones who appreciated all of these variations in soil formations because they were from a large country?

Jenny: Oh, yes. They were in many ways way ahead. Of course, they argued with each other endlessly. I remember that once in Missouri, we were there in the large pit and they had arguments. Finally, they asked the local leader, "What is the rainfall here?" Professor Miller said, "Forty-one inches." The Russians said: [imitates accent] "Impossible, impossible!" [laughter] It seemed impossible because they were fooled by the inches.

Maher: Did they think he was talking about it in centimeters?

Jenny: Yes. They thought it was centimeters. We had that same thing later in Saskatchewan, Canada. We saw these black soils and there was this Professor Penck. He looked around and asked, "What is the mean annual temperature here?" Somebody said, "Thirty-two." He said, "Ah, so hot! So hot!" [laughter] It didn't occur to Europeans that it was fahrenheit instead of centigrade, or celsius, but after a while they caught on.

Maher: Amongst the Europeans, the Russians were the ones who had already learned the lessons of the broad soil changes across a continent?

Jenny: Oh, yes, they were the leading people. Of course, they tried to explain everything in terms of the Russian climatic scheme. In the Great Plains area, the rainfall curves and the temperature curves run at right angles. In Russia, the two run parallel, which is a confusing thing. Hence, the Russians had a hard time getting American soils lined up according to their climatic scheme. I would say even now that is not resolved. This really is an integration of the climatic gradients and which way they run.

Maher: Didn't other Europeans still need to learn the lessons and to develop an appreciation of these large changes amongst soils and then to study the development--the formation--that was behind it?

Maher: Do you think that had much of an impact on the Europeans or on others, or did it just roll off of them? Isn't it true that only a few people had had the chance to travel extensively to see such dramatic differences?

Jenny: I would say it was not fully appreciated. The paper I wrote afterwards on my study of the climatic arrangements of the soils in the United States, based on Marbut, and on the climatic variables I had been working with, was quoted many times in Europe later on. But I don't think it really took a foothold. Now, many Western Europeans on the trip didn't have enough field experience about soils, and there were so many new things that you learned. For the first time, you saw red soils, or a lime horizon, or a chernozem. There were so many new things to see. So everybody was absorbed in that. But in terms of integration of the whole soil spectrum, that wasn't there.

Maher: How about for the need for integration of all of those things?

Jenny: I don't recall that it was brought up.

We (the younger people) were mischievous on the trip. I remember that every evening a newspaper came out, a mimeographed newspaper, about what happened during the day. This assignment was given to the various countries. One day it was given to the young people and we had to prepare the newspaper. On the trip was the famous Sir John Russell of Rothamsted, who was an important and famous person. Whenever we had a banquet or a meeting, he would talk about Rothamsted. And we got annoyed at that. So we put a question in the paper that asked, "Why did we come on this trans-continental excursion? Was it to hear about Rothamsted?" [laughter] Well, that caused an explosion! Waksman came to me and said, "You people shouldn't have done that. I'm sure you did it! That's bad. You'll have the English against you."

What is interesting is that later all of these newspaper reports were published in a book, but not ours! Our report was left out. Well, there were all of these antagonisms, especially among the older people. You could see which ones would talk to each other and which ones would not, and then there were women on the excursion and some men were chasing women. We had hilarious times. But, in a way, it was a very tiresome trip. I've forgotten how many weeks it lasted. When we finally got to San Francisco, we were so fed up with seeing soil profiles, fertilizer trials and experiment station plots that we younger people went to a movie instead of exploring San Francisco.

Maher: One last thing that I am interested in is how Lipman came up with the idea of such a grand enterprise? Did he feel that this overview was something that was lacking in soil science and that to arrange it in this way would drive it in?

Jenny: The way Lipman operated, I'm not sure that it was his idea. I'm sure he called in Marbut and other people. His whole policy as director of the experiment station was to communicate and pursue ideas. He might have gotten the idea from Marbut, but he pushed for it. He did the leg work to get it done. But Marbut and other people had to select the sites and decide which way to go.

The great hero (at that time) was Curtis F. Marbut of the U.S. Soil Survey. At New Jersey, I was already perplexed by the conceptually old-fashioned American soil classification. Marbut chose the occasion of the tour to throw it overboard and to adopt the European, essentially Russian system. While the new words podzol, chernozem, rendzina, solonetz, et cetera, were strange to American ears, they were sweet music to the European guests. Marbut received daily accolades and applause.

In Utah, we were taken to a barren area consisting of Na-clay (sodium clay). It excited Western Europeans and Atlantic Coast Americans alike, for neither had ever seen it, though they had lectured about it as the arid counterpart of a humid, acid clay. Here the "diamond" of clays was for everyone to see and feel. Wiegner literally went to his knees to gather a handful to take home.

To me, the tour experience was a thrill and it opened a new world.

I was impressed when we went from Washington south and saw the southern red soils and a few weeks later the black soils in Canada. I searched for the connection between the two. There must be a relationship, I thought, but in Europe the landscape is all cut up. The Alps wreck climatic gradients and smoothness of land, whereas in the wide plains of the United States there they were.

The red soils of the South and the black soils of Canada were showcases of the climatic theory of soil formation. No doubt, our European textbooks by Ramann and Glinka were right in principle. I was intrigued by the 1,000 mile-long smooth transition of the Great Plains area, without interruption by massive mountains à la Alps. There the rolling plains, I fancied, must harbor the secret of mathematical soil functions. At times I could hardly sleep thinking about it.

Maher: Did you discuss it with anybody?

Jenny: Maybe I did. I don't remember. I had a lot of contacts with a fellow, I think, from Czechoslovakia. Ladislov Smolik was his name. He died early. Oh, yes, there was a fellow, whose name was Adolf Reifenberg and he was from what is now Israel. I had met him already in Europe when he was a graduate student in Germany. I

Jenny: was with him a lot. Probably we discussed many things together. He was perhaps one of my closest companions. I would have to look up who else was around.

There was the feeling of being overwhelmed by the magnitude of the continent, and by the magnitude of the soil problems. We'd go to all of these stops and see all of these profiles, but how they were inter-connected, that was missing. Marbut tried to do it with the Russian climatic scheme. Later, after the tour, I think that lack of information on the connections pushed me to try to find solutions, especially when I started teaching. I had to somehow find a scheme to do this because when I took a course at Missouri, the first soils course was merely a recitation of various individual soil areas. There was the New England province, the Ohio province and the Middle West, but there were no connections.

Hilgard had made climatic connections half a century earlier, but Hilgard was an unknown quantity. I asked people in the United States about Hilgard, but they had never heard of him. Europeans had heard of him, but I don't think I met a single American at the congress who had ever heard of Hilgard. But I didn't ask everybody.

Maher: That's amazing. Did you meet Bradfield then?

Jenny: Bradfield was on the trip. He had developed the idea of soil acids, of acid clays. Of course, that was against Wiegner's ideas and my ideas, so both Wiegner and I were negative about it. But he expressed the desire to spend a sabbatical year with Wiegner in Zurich and he offered me his lab at Columbia, Missouri, during the interim, an opportunity I grabbed with both hands.

I never accepted his idea of a clay acid, but I learned a lot of things from his theories. I then tried to apply my thesis work to his clay and what came out is that it disproved his ideas about an acid clay and disproved my findings in Zurich which were done with permutites, not with actual clays. In my mind, that produced a sort of revolution about the nature of clays and soils. But I may want to add more about that later.

Some Thoughts on the Value of Historical Review

Jenny: A history of soil science, little segments have been written here and there but you would say by amateurs. It's usually not recognized that you need a training in history. So much of history is merely anecdotal. As I am thinking about this development, I'm wondering now whether what I am saying, how pertinent it is to the overview for an historian. I have, however, one inkling which makes me more favorable to this project than I was at the beginning. A

Jenny: friend of mine, who is a historian, wrote a section on soils and that was based solely on historical research of books and newspapers and literature and so forth. I could see right away that many major points were missed, or connections were not made about why soil science developed the way it did, compared to other fields in agriculture. So I can see that evidence like this oral history has a place, or may guide a future reader to other aspects and to other references which might be overlooked otherwise.

Maher: I think you have hit the nail on the head.

VII THE UNIVERSITY OF MISSOURI, 1927-1928

[Interview 5: March 31, 1980]##

Positive First Impressions of Columbia, Missouri and the Farming Community

Maher: Will you please start by telling how you got to Missouri from New Jersey? What was the timetable there?

Jenny: I must have gone by train. I know I went to St. Louis and there I went to the Swiss Consulate to register myself. At that time, in St. Louis, I didn't know that Hilgard grew up just across the river. Otherwise, I would have gone to Belleville, Illinois, where he spent his youth. So all I can say is that I came by train.

Maher: What were the circumstances that led to your coming to Missouri?

Jenny: The circumstances were that during the congress meetings Bradfield decided to got to Wiegner's in Zurich, and he invited me to take his place in Columbia, Missouri, while he was abroad for a year and I accepted with great pleasure.

Maher: By that time you must have hit it off very well with Bradfield.

Jenny: Before the congress, Bradfield had the idea to take a year's leave of absence. He was impressed with Wiegner and what Wiegner and I did. So he decided to go to Zurich. I'm just guessing, but I assume his leave of absence was on half-pay because I got some salary at Missouri. So that was very agreeable.

Maher: This was an agreement for one year?

Jenny: One year, yes.

Maher: This was what year?

Jenny: That was in 1927, the year of the congress. It was the big year. the year of Waksman, the year of the International Soil Congress, the first year in Missouri. The congress was held during the summer in June and July. I got to Missouri in September or October.

Maher: So the arrangements were actually made pretty swiftly, weren't they?

Jenny: Yes, that's right.

Maher: Before we leave New Jersey, this struck me. How did you feel about the time that you had spent in New Jersey?

Jenny: Do you mean looking back, or how did I feel at that time?

Maher: At that time how did you feel?

Jenny: Everything was so new when I left New Jersey.

In Missouri, in the great Middle West, I soon realized that the year spent on the Atlantic Coast had been confined to a small subculture. I had belonged to Waksman's international set; with my former Zurich roommate, Geering, an industrial chemist, I had frequented Swiss activities in New Jersey and New York. Here, at Columbia, I was in the heartland of America, surrounded by descendants of famous pioneers.

On my train trips to and within Missouri in the fall of 1927 I looked in vain for farmers and women and children working in the fields. In contrast to Europe, the landscape seemed abandoned. The small city of Columbia appeared as a compact, busy place surrounded by vast stretches of no-man's land. It seemed overly blessed with churches and chapels. The tallest structure was not the steeple of the biggest church but the Trust Company's bank building. North of town was the ghetto housing of the many black servants. Cleaning establishments were run by Japanese and the laundries by Chinese. American non-whites were not seen on campus or in classrooms, except as janitors.

I was put up in the house of the graduate fraternity, Gamma Alpha. This living experience in a group was totally new to me and most constructive. Everywhere, people were extremely friendly and eager to help. It was sincere, traditional American hospitality, though some eager beavers tried to make me join this or that church or club or to sell the greenhorn more insurance. Anyhow, I felt that I was genuinely welcome. Some of the friendships formed have lasted to this day.

It was like living in a new world. It had a strong, positive note, although, naturally, if you go from one culture to another some negative feelings existed. I had a great many positive experiences. Yes. And one prominent one was the kind of people

Jenny: there. The people were much freer and I noticed this especially in talking to farmers. They had a different behavior than many farmers have in Europe. They, I might say, acted as "free men." That was the impression I had.

Maher: Could you give some examples of that or be more specific?

Jenny: Well, I felt they showed great pride and, of course, what I didn't know then was that the idea of being the American pioneer was still in those farmers. It showed in the way they walked and the way people treated them. The university people treated them as equals and with respect, and it wasn't the same way abroad. In Europe, you still had somewhat the idea of the farmer as a peasant and perhaps of the upward mobility of the rich city people. Here there was less of a social stratification.

Maher: Did you notice any difference in that respect between the farmers of New Jersey and then Missouri?

Jenny: I did not meet many farmers in New Jersey. No. In fact, I don't recall any. I hadn't been on a farm.

Maher: Did you feel that you had accomplished some worthwhile work at Waksman's laboratory or was it kind of a side trip?

Jenny: No, I hadn't done anything, say, that I could publish, no well-thought out piece of work, because I shifted from Waksman to Shive, and then came the congress period. In other words, the time was too short. By the time I had learned enough English to communicate to some extent, the time was up. So it was a fragmented achievement. There were a lot of positive feelings and it pleased me to be there. I had no desire to run back to Switzerland.

Maher: So now, realizing that your academic or career work at New Jersey had been a bit fragmented and you hadn't really gotten your feet on the ground, what was your outlook as you saw it then when you went to Missouri for a year?

Jenny: I had no special plans. It was the idea of learning--getting into new things, seeing new soil, new worlds. I wasn't really concerned with writing something or publishing a paper, as I would be now.

Maher: Did you view yourself as a student?

Jenny: Well, not exactly as a student but as a traveler or an explorer. No, I never thought about a publication or anything like that. I had just published a book in Zurich with Wiegner, also another book with Braun-Blanquet and I had my thesis published. So I had no intention of publishing. Yet before I knew it, this same fall, I had a paper ready for publication.

Jenny: The tumultuous year of 1927 culminated in the presentation of my first nitrogen paper at the meeting of the intellectually vigorous American Soil Survey Association held at Chicago on November fifth.

Shortly after the Chicago meeting on November eighteenth, 1927, I married Hedwig Schlaepfer, from Zurich, in Professor Miller's home. She was a nurse and violinst whom I met at the Swiss Club in New York and she was an x-ray technician at Hackensack Hospital, New Jersey. We planned to stay a few years in the United States and then return to Switzerland. But fate ruled differently.

Research on the Relationship of Climate to Nitrogen in Soil

Jenny: Still fascinated by the summer's transcontinental experience I kept thinking about the contrast between the dark-colored Canadian prairie soils and the lighter-colored counterparts in Missouri. From bulletins and unpublished reports, I gathered soil nitrogen and carbon analyses and arranged them in tables and graphs. Within a few weeks, it became clear that, from Canada to Louisiana, along a temperature transect having similar moisture conditions, soil organic matter was sharply declining in exponential fashion. Well wishers encouraged me to present the findings at the upcoming meeting in November in Chicago, which I did, as already mentioned. Almost everybody seemed impressed, especially Marbut, who became a friendly counselor. The response spurred me on to search for additional, supportive Nitrogen-Temperature (N-T) functions.

Russians long had known that, in their country, humus is linked to climate, a trend they attributed to moisture stress, because cool, wet countries dominate their north and hot deserts their south. The transcontinental tour taught me that, in the Great Plains, area temperature and moisture trends are not synchronous, as in Russia, but intersect at nearly right angles, which provides an ideal climatic checker board. Later, I made the most of Nature's fortunate American design.

Maher: Let's get onto probing into the background and influences on that first paper for the Chicago meeting, 1927. How did things unfold intellectually once you got to Missouri? How did you settle in and begin to figure out what you would work on and what you settled on?

Jenny: I was well received. Everybody was friendly and we had a lot of discussions.

In Missouri, I was an instructor in the Department of Soils but I was working with chemical processes, a combination that was a novelty to me. In central Europe soil science was taught by the professor of agricultural chemistry and the scope of the lectures

Jenny: reflected the docent's personal, often one-sided, interests. Here, an entire staff dealt with the extensive domain of soil science and did it on a broader scale than I had seen at New Brunswick.

I attended Professor M.F. Miller's lectures on soil classification which centered on the soon to be abandoned system of soil provinces. Pedologist Professor H.H. Krusekopf took me along on his many trips to the loessial prairies and the forested Ozarks and taught me soil surveying, which then still relied on the plain table. To satisfy my curiosity about the land and people, I cross-examined and pumped him relentlessly in field and office. Soon he became my pedological guru and a personal friend. Before long I teamed up with broad-visioned Professor W.A. Albrecht, bacteriologist, and, in the green house, grew soybean plants on mixtures of sand and colloidal clay.

When I asked these colleagues about what I saw on the big excursion--that the soils in Canada are black and in Missouri they are yellowish and light brown--I did not get a clear-cut answer. I suppose people didn't know. While on the excursion, I started speculating about the climatic principle. So from the climatic background I had acquired in Zurich, I started looking into it. I got analyses from the literature between Canada and Missouri and points south and plotted the nitrogen content against temperature, keeping the rainfall somewhat constant, and these curves showed up. In a very few weeks, I had two curves with a lot of points on them. I showed them around and discussed them and people were impressed. They said I should present this material at the meeting of the soil survey group in Chicago, a group that had some very active people in it and that showed a lot of curiosity. Of course, I had been stimulated by the whole excursion and the changing soil classification ideas. So that was a fertile ground on which to present this sort of material.

Maher: You had really plunged into it right away.

Jenny: Yes. Right away. Probably in the first two weeks or so I started because Krusekopf said, "We have nitrogen analysis, we have carbon analysis," and he gave me hints on where to look for them, and I got information about the various state experimental stations in the United States, how they are organized and what they published. For example, bulletins. So I got all of that and it was exciting.

Maher: Going back in a little bit more depth to what you had been exposed to in Europe in terms of the Russian climatic school of pedology, could you explain in a little more detail and depth about the thinking you inherited and what were the avenues of thought in the books that you had read or the individuals in Europe that you discussed this with?

Jenny: I heard about the Russian views of soil science in the lectures on soil science by Wiegner. He presented a beautiful panorama of what the Russians claimed was occurring in nature--namely, based on their Russian conditions, that soils were molded and formed, essentially by climate, and that they had established a climatic school. That's what I learned. Then Wiegner had a graduate student by the name of Meyer who tried to quantify this climatic pattern. Earlier an attempt had been made by [R.] Lang to show that certain soils lined up with the ratio of rainfall to temperature, the latter simulating evaporation. I had talked to Meyer a lot. He found the ratio insufficient and he brought into the picture the dryness of air as a substitute for evaporation. Hence I had been exposed in Europe to the idea of somehow combining precipitation and evaporation to a soil moisture index. So when I came to Missouri, these notions were already in my background, but temperature itself, as a soil forming factor, was not in the picture.

Maher: Had you known the gentleman, Lang?

Jenny: No, Lang was a professor at some German university, and I didn't know him.

Maher: Was it clear how the Russians were distinguishing between the effect of precipitation and the effect of temperature or temperature and evaporation?

Jenny: As I recall, there was no real attempt by the Russian pedologists to pinpoint it with temperature or moisture. Their idea was that moisture was the great climatic factor, because in Russia, in the north, they have high rainfall and as they go south toward the desert, it gets warmer but also drier. So they had this enormous moisture gradient and the soils lined up pretty well on that moisture gradient.

Maher: But it was at the same time a temperature gradient?

Jenny: Yes. The temperature got higher as you went south and the rainfall got lower and these two trends reinforced each other. But, in Western Europe, where Meyer was trying to figure out the climatic scheme, it wasn't so simple. Meyer didn't stress the temperature. But when I came to Missouri I suspected that temperature might be very important.

In fact, my first work in this transect from Canada south to Missouri, was really a temperature thing. I remember so well how this temperature phenomenon affected me and why I thought of it because, when we were somewhere in Alberta or Saskatchewan and saw these black soils, it was cool. In Missouri it was still plenty hot when I got there. So all of these things combined to impress me about the importance of the temperature component in this climatic scheme.

Maher: As we move into your years in Missouri, I'm curious about how your work in the Alps perhaps helped build the foundation?

Jenny: Well, you have a point there because in the publication on the soils in the Alps in the Swiss National Park, published with the ecologist, Braun-Blanquet, I had already drawn a soil climate function or a curve relating humus production to rainfall or to a humidity index. That was very speculative. Incidentally, when I did that curve I used the data which Hilgard had published in his book and that's how I became acquainted with Hilgard's work.

I assume the Americans didn't come up with this climatic scheme of soil organic matter because they had not been exposed to the Russian school. Only through the congress, and on this international excursion were they confronted with it. Before that, they had their own soil scheme of geographic provinces, and the climatic picture was not in it.

Maher: This, as I understand it, had been developing mainly as a practical soil survey matter in the United States. Is that correct?

Jenny: Yes and no. When Milton Whitney started the soil survey, he had a talent for getting good people. Among the soil surveyors, he had two geologists, and I assume that through their influence the idea of geographic provinces, which were really physiographic, came in. Incidentally, Marbut was a physiographer working early in the soil survey.

Maher: So when you talk about the system of soil provinces in the United States that was soon to be abandoned, it seems evident that what you're saying is that because of this international conference there was really a large tumult about how to approach soils on the broad view. Marbut, I gather, was trying to change the U.S. system.

Jenny: Marbut was in a difficult position. On one side was what he thought about the soil and on the other side was what the government thought about the soil. He was confronted with politics. There was this big soil survey scheme and these soil survey people were not going to change their thinking right away. So I'm sure Marbut had formulated this idea of going over to the Russian scheme quite a bit before the excursion, and that he used the excursion and the selected soil sites to bring that into focus. For all of the Americans on the trip (there must have been, I don't know, thirty or forty Americans) this was a new thing. I know Bradfield was impressed but M.F. Miller was not. So the younger people were more or less for Marbut, but the older people who were entrenched and had administrative positions were not keen about it. So there was this conflict which appeared on the trip quite often between Marbut and [A.G.] McCall. McCall was in charge of all of the soils work and I guess he was Marbut's boss. He was very conservative and we had the feeling that

Jenny: he opposed Marbut. That was the feeling among the young participants. I don't know exactly how that happens, but I can readily see the way politics works. It was the situation then.

Maher: So there was kind of a young vanguard that was really receptive to new ways of looking at soil and the idea of basing soil classification on principles of origin, and generic types of views. Were these young people the main ones who were enthusiastic about your work and giving you reinforcement or was it pretty much across the board?

Jenny: No, I wouldn't say it had to do with age. All were receptive to the curves I had drawn. Marbut got interested in the Russian scheme through reading the book by Glinka, which had appeared in German. I think Marbut translated that book into English, if I recall, correctly. The problem Marbut had--probably before the trip--was convincing the various soil surveyors in the various states. Charles F. Shaw, in California, in charge of soil surveys here, really knew more than Marbut about the California soils and he could point out cases of soils that did not fit the climatic scheme. This was one of Marbut's difficulties in laying out the excursion's itinerary. There were just too many exceptions which the local people knew. Then, of course, there were other factors that were not known. The age of the soil and the topographic picture were not fully explained in the Russian scheme. On the trip, Marbut had great success with the Europeans but no so much with the Americans.

Maher: How did you feel about these inconsistencies? You had been struck very much by the change in soil color from the dark Canadian soil--

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Maher: --to the light southern prairie soil, this being kind of a continuum which is intellectually very attractive and I'm sure it was to you. How were you impressed with the inconsistencies that were being shown by people like Shaw?

Jenny: I would say that for the period we are talking about within the Missouri period, in the very late twenties and the early thirties and on that great trip, I had not been made aware of these discrepancies because all of our stops were in the areas that fit the climatic scheme. So I didn't know about that, although at New Jersey I was shown the Sassafras soil series and it looked to me like some sort of podzolic thing. But I sort of pushed it in the background. It made no sense because podzols were way north, as in Scandinavia. So I didn't know what this meant.

Jenny: In Missouri, I was exposed to soils--for example, the Lindley and Marshall series--that were difficult to place together in the Russian scheme. In fact, through my nitrogen and carbon work I came slowly to the impression that the Russian scheme was too narrow, too one-sided, by emphasizing one factor, namely climatic moisture.

Modeling: Developing the Mathematical Curve and Equation

Maher: Through all of these peregrinations we come to this tremendous period which was very short--September and October. You're getting together all of these figures, the bulletins from the different experiment stations and drawing together a paper to present in the middle of November. You mentioned that people were encouraging you to do that. Who were some of the people other than Krusekopf?

Jenny: Well, Krusekopf and Albrecht and also M.F. Miller. Another thing I can see now that impressed them was that when I plotted the data, I got a curve and then I developed an equation for that curve. Well, that was new to them. Then another thing that impressed them was that while I was at Zurich working with Braun-Blanquet I had become interested in statistics. So I knew about the Gaussian Curve and I knew about the significance of a mean and other statistical considerations. It was mainly based on a book by a Scandinavian, W. Johannsen, I believe, who was a botanist who did a lot of work on statistics--or a geneticist, I think he was a geneticist. He gave a good presentation of statistics. So I did that too. So there was really a neat package. Everything was new. The date for submitting titles had passed so they must have squeezed me into the program.

Maher: It has often struck me from our discussion and reading your papers and reading what you have written on Missouri that there was a certain charm in these mathematical curves. Could you give me an idea of the feeling behind that, the strength of that urge to try to find a mathematical curve that would be a description of this?

Jenny: I don't know how my liking of mathematical curves really originated. We had a lot of math in high school. At the same time we were confronted in our art courses with modern art, such as Cubism and Expressionism, and there forms played a big role. If you think of Kandinsky and Miro and all of those painters, they had strong lines, and I liked that. So probably it was a combination of my background in art from woodcarving and family traditions, the mathematics I had, and the interest in modern art. All of these probably brought into focus the interest I had in lines. I always did a lot of sketching in nature, landscape sketching in black and white. So lines were important to me. So I guess it was a cultural thing that got me interested and, when I could see curves in my work that was just great.

Maher: In the development of this work from the summer--the period of actually being in Missouri and collecting the data--at what point were you beginning to realize that a curve was applicable, that there was going to be this aesthetically appealing way of expressing nature?

Jenny: Well, I don't remember but I must have plotted the data with a horizontal axis and a vertical axis. I plotted the points and the points lined up so I could see a trend. I not only tried to describe the curve by some exponential formula, but I was also interested in how the curve could have originated. At that time, I often used a graph that had been published by a microbiologist, E.H. Panganiban, showing the effect of temperature on nitrification or carbon dioxide (CO2) production. It was a beautiful S-shaped curve. So I used that sort of information to understand why soil behaved as it did. Apparently, I wanted to be sure my curve was not just a coincidence but had some theoretical background to it. While I was convinced much earlier that temperature increased soil microbial activity, I was not sure how temperature influenced the production of organic matter. And that's still a problem that needs a lot of work because it's tied to photosynthesis. The real equation for combining production or organic matter by photosynthesis and decomposition by the soil microflora and fauna has not yet been satisfactorily integrated. So there is still a lot of important work to be done.

Maher: You point out that you brought to this study skill in curve-fitting and you also brought to it your background in statistics. Both of these you characterize as fairly new ingredients, at least in Missouri. At the same time (and this is getting a little ahead of things), you mention in your writing that when you actually got to the point of publishing, especially later on in the Journal of Physical Chemistry, several pedologists accused you of being erudite. What was going on here in the reaction of Americans to what you were publishing or to your ideas that developed into this mathematical form?

Jenny: There is a legitimate question, say, to a field surveyor who asks, "What does this contribute?" What does it tell him? If you just plot the graph and have the points of reference, that's really the basic information and he can see that. So why go into formulating the thing? Bradfield defended me at meetings when this came up and he said that the mathematical approach really does not open up new vistas but it presents the empirical material in a more readily accessible form. So if you have an equation, all of the data are concentrated or condensed in that integrated symbol. So it is, you might say, a superior form of bookkeeping. That was Bradfield's idea and I accepted that but I had more in mind--and that is now known as modeling. That word was not known then, but essentially that's what it was. That came also from my background in Zurich and in my training in chemistry and physics and so I thought about where

Jenny: you should set up models. We talked then about theories in trying to understand what was going on. That still interests me. This is still important. So one might say I was modeling at that time in a primitive way.

The University of Missouri's Stimulating Scientific Faculty

Maher: You also have mentioned that at the university, you were encountering stimulating discussions with people on the faculty.

Jenny: Although Columbia, Missouri, lacked the international reputation of Zurich it was, in turn, spared the exclusive cliques of prima donnas of science. Instead, the university's stimulating faculty fostered intercommunication. I was close to [Samuel] Brody (animal physiology), Kirby Miller (philosophy), [William J.] Robbins (botany), Schlundt (chemistry), [Lewis J.] Stadler (genetics) and [Allen E.] Stearn (physical chemistry). The favorable environment encouraged my professional growth. It proceeded along the lines of pedology and soil colloid chemistry.

Maher: You mentioned Brody and Kirby Miller, a philosopher, Stadler in genetics, Stern in physical chemistry, and Robbins in botany. Did you speak with them about these emerging ideas and the things that were impressing you?

Jenny: Okay, now take Robbins, who died a few years ago. He left Missouri after I left and became director of the New York Botanical Garden. But he had worked at Missouri on the Donnan equilibria with plant tissues. So he had an interest in these colloid chemical aspects and, when he heard that I was involved in these things, he gave me his papers.

I remember one afternoon he asked me to come over. He wanted to show me the Biological Institute. I remember one scene. We were walking down the steps and on the wall was mounted a collection of snail shells by a former faculty member, Guthrie, and the collection emphasized evolution of snail shells. There was a date on it and it said 1850 or something. I said to Professor Robbins, "Well, this was done before the Origin of Species was published by Darwin." I could see right away that Robbins was impressed with the sort of soils man who would know about Darwin and the date of his important book. So he smiled and from then on he was a promoter of my work and he helped me in many ways. Yes, in Zurich, after reading Haeckel, I read Darwin's book.

Maher: What, incidentally, was his answer to your question?

Jenny: Oh, he said, "Yes, yes, that's true." Yes, but what this fellow Guthrie didn't do was formulate a broad principle, a broad system. It was too isolated an incident and probably there are many more before that and now gradually the literature is coming up with finding suggestions that Darwin had received from earlier workers.

Maher: Did you talk with a man like Robbins about the pedology end of your interests?

Jenny: Not much, no, although he was chairman of Sigma Xi and he invited me to give a lecture on my work.* He was interested, but, no, we didn't discuss it. The fellow I discussed it with was Stadler who was a geneticist. I got a lot of information or shall I say "acquaintance knowledge" on genetics from him. He was a very keen fellow and he was interested. We lived in the same direction so we walked home together and he asked me what I was doing. He had a lot of questions to ask and we became quite close socially. Unfortunately, he died early.

As to Brody, we had a lot of common interests, because Brody was working in animal husbandry and, in Zurich, of course, Wiegner's agricultural chemistry included animal nutrition. So Brody and I knew of Wiegner's work thus we had a basis for discussions. We were together a lot, often arguing about things, and Brody was very broad. We had many discussions together while drinking coffee.

Maher: Did you find that minds like these were comfortable with your expression of your ideas in terms of mathematical equations or even your speculations? Were they comfortable with your usage of statistics?

Jenny: Well, I would say statistics was not important then. Statistics came up while I was in Missouri. In the Middle West, the work of the English statisticians (e.g., R.A. Fisher) became prominent. Iowa then became a center of statistics teaching, and people from Missouri went to Ames, Iowa, to get training in statistics. So, at least, I could talk to them, although the idea of the "degrees of freedom," which was the new thing that came up, was to me unknown. But this was an important development because it permitted using the statistics of small samples. The old statistics had to do with big samples and I was taught from my reading that you should never compare averages unless you have thirty samples in each. By this rule of thumb you didn't have to worry about "degrees of freedom," but you had to do a lot more work to get the data. But the formulas were the same. The T-test was not known. Essentially, it was a

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*Sigma Xi is a student science society.

Jenny: test in which a difference was significant when it exceeded three times the mean error. So this was all sound, except it forced me to get more samples to overcome the statistical limitations.

Stern was a professor of physical chemistry and he was interested in Donnan equilibria and electric double layers and colloid chemistry, so we had lots of discussions together.

Kirby Miller was a professor of philosophy who was interested in, you might say, a philosophy of science, and the scientific aspects of philosophy in relation to science. He conducted seminars and I participated in those seminars. Then, I was reading a book which I found very instructive on the philosophy of science. It was written by N.R. Campbell, an Englishman. It was a big book, and strangely enough, there was not a single reference noted in it because Campbell wrote that he had lost them all. The book discussed such questions as: What is law? What is a theory? What is a hypothesis? I found this to be very stimulating. So I presented a seminar about this kind of thinking. Through Stadler, I got interested in discussions on evolution. He conducted seminars and he induced me to participate.

I now had this reputation that I could do curves and equations and pretty soon they got the idea that I knew much more about mathematics than I did. Fairly soon at Missouri I started to take courses in higher mathematics. I took a course in differential equations. So I got that far. But I never had enough mathematics. They thought I knew much more, but it wasn't so.

The Trip to Kansas and Nebraska to Study Chernozem Soil: Further Development of the Nitrogen-Climate Equation

Maher: Going from the theoretical end to the empirical end, at what point did you develop this desire to go out into Kansas to test and see what you could find? Was that in this early period or was it after the November presentation?

Jenny: I would think it was after the November presentation because, before it, I studied the soil nitrogen literature. Soon I became acquainted with Marbut's new American system of soil classification. I compared it with European systems and finally wrote a paper in German on the comparison of European and American soil classifications and on the similarities and dissimilarities.

In the center was chernozem, the great Russian climatic soil type, and from temperatures and the NS-quotients of Meyer, I figured out its American counterpart must be in Kansas and Nebraska. So I was eager to check it out. When I had a chance, I took a trip there

Jenny: and had a look at it with a fellow by the name of Roy Hockensmith, who later was an administrator in the Soil Conservation Service in Washington. We drove the car we had been assigned, an old Model-T Ford, westward, deep into Kansas, at the 25-miles per hour it was capable of doing. We found the missing link and were elated, having seemingly confirmed the climatic principle of soil genesis. Alas, back in Columbia, we were lambasted for having taken a state car across the state line without prior permission.

Eventually, I acquired a substantial collection of soils from places across the Great Plains, along a transect of increasing mean annual precipitation but constant mean annual temperature. The soil organic matter responded to rainfall logarithmically. The individual moisture and temperature relationships enabled me to formulate an "equation of state," a three-dimensional nitrogen-climate (N-climate) surface, which was published in 1930 in the Journal of Physical Chemistry. I enjoyed seeing field data aligned by equations and derived aesthetic pleasure from the shapes of the curves. Several pedologists however, accused me of trying to be erudite.

Maher: That must have been a tremendously gratifying experience when you finally got there.

Jenny: Yes, when we saw it. Yes, it was. But already I realized right then that in this undulating loess landscape the typical profiles were only on the level portions, not on the slopes. That was a different situation and you could see that the slopes had been eroded. The black soil was gone and the yellow subsoil was showing up. So I realized then that if I wanted to get the true climatic effect, I had to confine myself to the level crests of the loess' undulating landscape. In that way I kept topography "constant," although that concept wasn't clear to me at that time. Later I realized that's what I had done and so I put topography into the equation.

In the same year, I wrote an extensive overview for Research Bulletin 152. During the absence of the Department Head, M.F. Miller, in Europe, I smuggled in a discourse on the low corn yields in the South, attributing them to the scarcity of nitrogen rather than to summer dry spells, as was customary. Miller had informed me that he thought the thesis was too speculative. Two decades later the idea was given credit in farm journals, and in Bulletin 366 (1949, North Carolina Agricultural Experiment Station, Raleigh, North Carolina) for having helped foster the spectacular rise in corn yields that followed on the heels of advances in hybrid corn genetics. For my friends abroad I wrote an account, in German, in the "Naturwissenschaften" but Wiegner and his staff kept mum.

Maher: Let's go to the presentation on November nineteenth in Chicago. What was that Chicago convention like? It was for the American Soil Survey Association.

Jenny: Some key members were there that I got to know later. One was G.W. Conrey from Ohio. One was a fellow in Indiana, Tom Bushnell, and a fellow in Nebraska, a keen fellow. What was his name? It escapes me at the moment--J.C. Russel.

Then there was Marbut who attended for an hour or more. He gave a talk about the congress and the field trip--the excursion. As I mentioned once before, I was startled with what he had to say about Wiegner and me. He did not know I was in the audience and he said he thought the best paper presented in Washington was the one of Wiegner and Jenny on ion exchange. But he added that wasn't soil science and to me it was just fantastic to hear that because I had been taught that ion exchange is the basis of all soil reactions. But later I came to realize that Marbut really had a point, because soil science is much broader than specific chemical reactions, although ion exchange reactions really dominate soil chemistry.

Complexity of the Soil, Changing Theories, and New Research Tools

Maher: Is this a matter of distinguishing between the level of soil forming processes and the overall forming factors and end products?

Jenny: Well, I would say it this way. You go into the field and you collect a soil sample, assuming that this collection has meaning in terms of soil in nature. You take the sample to the lab, you soak it in water and disperse it, centrifuge it and you end up with clay. Then you study the chemical reaction of that clay. However, that's not the chemical reaction in the soil in the field. In the soil in the field, you have all of the organisms and earthworms and whatnot and these are not in this laboratory study. So it is a very specialized, miniature component of the soil, yet I'm sure it is important. But there are so many other things like soil biology or soil microbiology.

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Maher: How were you beginning intellectually and personally to confront this growing awareness you must have had about the ungodly complexity of soil?

Jenny: Well, I would say that it started with a bang in New Jersey under Waksman. In Zurich, Wiegner was a great personality and very powerful and influential in terms of colloid chemistry. In a way,

Jenny: Waksman was also an intellectual and a powerful person in terms of soil microbiology. In his lab I was exposed to the very drastic statements in which he said, "There is no chemical reaction in the soil that is not initiated and catalyzed by bacteria or fungi"--a very drastic statement, that was followed by one shock treatment after another. Then came the experience in New Jersey with the soil survey. So it became clear to me that my ideas of soil were really narrow although they were broader than Wiegner's, because I had worked with Braun-Blanquet in the Alps and had discovered the alpine podzols. So I think it was a gradual experience, but I would say essentially an American experience.

Maher: So you were being confronted as your career progressed with very strong statements by intellectually respected people with fairly specialized views of soil. Meanwhile, you were getting all of this experience from overviews, from crossing the North American continent, and developing this set of figures across the prairie. You were seeing that there was a very large matter that was very stubbornly being boiled down to a few bold statements. What did you feel that you were going to be able to do intellectually to accommodate all of this complexity?

Jenny: Probably I was just struggling like a fellow in the foxholes on the front lines, just fighting left and right trying to survive without broad philosophical speculations. I still think if you want to be productive in research you have to be immersed in it and struggling and the illumination will come later, if at all.

Maher: The generation of data, the rounding up of that data.

Jenny: That's right and also while I'm greatly impressed and like theories and modeling, the history of soil science has shown that a new instrument may lead to collapse of beliefs. So new instrumentation is another approach to understand nature and modeling is related to the production of new tools.

Maher: Do you think that you were at any point kind of enchanted by the idea of being able to describe the soil forming processes, the dynamics of soil, in a mathematical way?

Jenny: Later, in Berkeley, when I developed a soil forming factor equation, to me that was great, yes.

Maher: But not by this time in Missouri?

Jenny: Well, not in that general way, no. It was just a more specific way of how the curves ran in these various areas.

Maher: You were originally to stay for a year in Missouri.

Jenny: Yes, I was to stay for a year.

Maher: You were pretty much free to do what you wanted during that year. Is that correct?

Jenny: Yes.

Maher: What were your duties over the ensuing period in Missouri and how did that balance with the development of your colloid chemistry work?

Jenny: Well, I had a few responsibilities. Bradfield had some graduate students and during his absence I had to assist them and be in charge of the laboratory and see what had to be bought and see that things were working. Also, Bradfield had bought an ultramicroscope which had never been put together and this effort was part of my assignment--to get that into working condition, which I did. But I would say the lab work in colloid chemistry during the first year was secondary to my interest in the outdoors and in the climatic picture. After Bradfield got back I started to work in the lab.

Maher: So you really did the pioneering work of your own in respect to pedology in that initial year?

Jenny: In that initial year I did a lot, and in sufficient intensity to keep the field work going when I got graduate students. Later, I kept working on these curves myself.

Coordinated University Efforts to Benefit Farmers

Maher: You mentioned that at one point you had a suggestion from Miller that you take a look at the effect of agricultural cultivation on soil organic matter. How did that fit into this whole experience. When did he make that suggestion? Was it in the first year?

Jenny: I don't think it was in the first year. I think it came later--I had published several papers on climate and organic matter. I don't know what made him suggest it, but your question brings something else to mind. What was new to me and that had both positive and negative elements was the fact that here in the United States, in Missouri and in New Jersey, too, the agricultural experiment station and the university were together--and the people belonged to both. Whereas, in most of Europe that is completely separate; they are in different places. The university is here and the agricultural experiment station is somewhere else and you have hardly any contact between them. But here both were together and that included, not only the experiment station, but also the agricultural extension service. So there was an awareness about how the work in soils could have meaning for the farmer in the fields. A head of the

Jenny: pertinent university department, like M.F. Miller, would look around and ask, "What is new or what ideas can be pushed to get money or funds?"

When I started at Columbia, Missouri, the department's interest was on liming. Soil acidity had been discovered and Bradfield had pushed it and developed it so the emphasis on applying lime was a big thing. But then I suppose Miller had to look for other things to develop because they were meaningful for farmers.

Tucker Prairie and Study of the Effects of Cultivation

Jenny: I don't remember how we picked Tucker Prairie, a relatively virgin piece of land, whether he knew about it or I had heard of it or what. That's the area we picked along the then Highway 40, and it contained about twenty or thirty acres of prairie land that had been grazed but never cultivated. I remember that when I saw this property and worked on it, I thought it should be preserved. I was interested in buying it, but my low salary of $1,800 a year did not permit that. But I'm pleased to say that the area is preserved now. I think that under the auspices of the university it has become a natural preserve.

What is interesting is that I took all of the samples there in a systematic, although not as well designed a way as I would do now. I took over seventy samples from the prairie and the same number from the cultivated field across the highway, both on level Putnam clay pan soil. I kept a book of records, a laboratory journal quite neatly put together. I should show it to you. I have it at the office.

We found that sixty years of farming had induced a loss of 35 percent of total soil nitrogen and a slightly higher amount of carbon. Additional comparisons discovered in the literature permitted the construction of a declining nitrogen-time curve (Bull. 324, 1933). Years later I developed a simple mathematical model of gains and losses of nitrogen (N) and carbon (C) that envisioned an equilibrium or steady-state condition of soil organic matter. It has gone through many textbooks and I think it is still being published. I got a request the other day to republish the curve.

Further work disclosed the secondary role of vegetation, topography and parent material. This revealed that I had "re-discovered" the five soil forming factors of Hilgard and of the Russian Dokuchaev School. Still, the nitrogen-climate surface work qualifies as an original contribution to the storehouse of pedologic

Jenny: knowledge. The logical extension of the factor scheme to the clan of pedogenic functions did not emerge until the subsequent California position enhanced my self-confidence.

Comparing Todays Soils with Research Results Fifty Years Ago

Jenny: Now, at Missouri, fifty years later, they are re-examining what I did and I suggested that they should re-analyze this virgin prairie to see whether it really is in a steady state. A young woman, a graduate student, Alice Walters, is working on that. I sent her my records that show every spot where a sample was taken. She wrote me back that she was quite thrilled and showed the notebook around as to how complete a record we had for that study. So I will be interested in what's coming out. Also, this student is checking on my other soil samples collected over the state. I always had the soil survey maps with me and I marked very carefully where I took a sample so that record still exists. It will be interesting to see whether the organic matter studies I made represented steady states or not. That's an interesting development and, of course, it becomes important now, in view of the total global carbon system, whether soil organic matter is now in a steady state or not.

In fact, in a way, at that time I considered myself at a disadvantage because I thought others knew more about Missouri agriculture. I have too many interests. One thing leads to another and in a way it would be better to stick more to one thing. But anyhow, that's the way it is.

Maher: It strikes me that maybe this is just the way it tends to work out. That you are on a kind of pendulum in your own mind and your assessment of yourself and of others. Perhaps you are thinking about how to approach your work. On the one hand is the rigor and the dependability of staying in a small realm and developing your knowledge thoroughly. Then, on the other hand, there is the push for gaining the input from other points of view and bringing those points of view back and bouncing them off of this more thorough work, whatever it is.

Jenny: Yes. Well, that may be wishful thinking to say, "I should have stayed on these climatic, nitrogen and carbon things much longer and then I could have come up now with a better picture with this new thing." But it is not necessarily so because I might just have become stagnate and in a groove and perhaps not have seen any farther. So maybe it wasn't a bad thing. All I can say is that I had a good time doing all of these things. I really talked a lot

Jenny: with Overstreet about that--the purpose of doing research beyond the call of duty, and we finally concluded, "We are just having a good time and are being paid and we had better be quiet!" [laughter]

Maher: All right, getting back to this work on the cultivated and the virgin soil, how did that discovery strike you? What did it mean to you?

Jenny: [pause] It introduced me into a quantitative time picture of soil, of soil changes, how soils change with time, especially under the influence of man, and that you could get curves and where that would lead? What is the fate of the soil in such a scheme? But that point came later when I wrote the book and I started to think more about time functions. Then I took these Missouri curves and sort of re-invigorated them and discussed how the curve declines according to farm management practices depending upon whether you have continuous corn or hay or legumes or what. But I did not do that in that first paper. I did it in the book.

The Role of William Albrecht

Maher: I have one more thing that I would like to ask you about. What was Dr. Albrecht like? You have described him to me as a broad-visioned person. How did he fit into your experience?

Jenny: Albrecht has a very good reputation in circles of alternative agriculture. Albrecht grew up on a farm in Illinois and I think he majored in Latin and then switched to bacteriology. He was a professor in soil bacteriology at Missouri. Soon after I got there and could handle clays with Bradfield's technique, we started to prepare calcium clays with various degrees of saturation, with various proportions of acidity and calcium and growing plants, especially soybeans. He was interested at that time in calcium nutrition and since Missouri had mostly acid soils, there was the question whether calcium added to soil merely changes the pH or whether it has a nutritional effect also. So we worked together on that problem.

Albrecht also had a laboratory--not really a laboratory--but two big rooms that were devoted to inoculation of legumes. There, he was preparing inoculants, that is, test tubes containing gelatin and nodule bacteria to mix with legume seeds. This was sold all over the state and it kept a lot of graduate students busy and paid them. All of these graduate students were really paid by making these inoculants for the farmer, which were sold at a modest price. This was in connection with the extension service. With all of that,

Jenny: Albrecht became interested in plant nutrition, soil fertility, and eventually, how it effects animal nutrition and human nutrition. He had broad ideas, but I felt he was slightly on the superficial side.

For example, we got into an argument about my data which I didn't want to publish because I felt it was not secure enough. But he took it and wrote a paper about it. Well, probably I was too particular about it, wanting everything just right, a sort of perfectionist thing, which I still have. I would say no damage was done by publishing that paper.

Maher: That's the one that is under his name and yours?

Jenny: No, we wrote one together, and then he wrote one himself using my unpublished data. [pause]

He was a good church member, and a Kiwanis Club member, and a really good guy. He would go out of his way to help you and he was a very good Christian. In fact, sometimes, I felt I owed him too much. In that way, I was a captive in that I had to return favors. Especially at the beginning it was very good working with him.

Maher: Could you be more specific?

Jenny: When I came to Missouri, I arrived in the afternoon and I didn't know anybody, and he asked, "So, where are you going to stay?" I had no idea, you see, and he put me right into a fraternity house, Gamma Alpha. So I was taken care of there and there were lots of things he did. When I had to leave Missouri to go to Canada to re-enter with a different visa, he arranged for a lawyer for me. He had all of the connections needed. He knew everybody and he was very good.

VIII THE UNIVERSITY OF MISSOURI, 1928 TO 1936: SOIL CHEMISTRY RESEARCH IN THE 1930s

[Interview 6: April 14, 1980]##

Working with Robert Bradfield

Bradfield's Comparison of American and European Research Techniques

Maher: You said that Bradfield went off to Europe and you took over his lab.

Jenny: During his leave of absence he went to Zurich and also to Vienna.

Maher: I am curious about the news he brought to you when he got back. What was on Bradfield's mind when he returned? What had he learned about this period in the labs in Zurich?

Jenny: Well, at the faculty club, he gave an evening talk and I was, in a way, quite disappointed in what he had to say. For example, he said that Berlin and Zurich were more expensive than Columbia, Missouri. I thought, "Well, yes, but they are not the same thing either. There are a lot of things over there that don't exist in Missouri and they have to be supported and maintained."

Maher: He was referring to the university or the cost of living?

Jenny: The cost of living in general. He was quite critical of what he saw abroad and so I guess that was one reason I wasn't enthused about what he had to say. I would have to think more specifically about what he said, but one thing he thought was that the way they do research and write papers was too speculative. He criticized Wiegner for always wanting a clay model. Bradfield thought it was more important to study different clays in nature. I didn't fully

Jenny: appreciate that comment at that time, I would think. One approach should not be done exclusively of the other. Both should be pursued.

He also remarked that they ran experiments and then wrote a paper. I don't know whether he was referring to Wiegner's lab only or the short period he spent in Vienna, and I think he went to Berlin, too. He elaborated on this, noting that at the beginning of the paper they set up a sort of a theoretical introduction, a theory or a model, and then go ahead in their writing and claim the experiments prove that model. Whereas, Bradfield said, correctly, it was done the reverse way. They were studying the reactions and arrived at the model or a picture, and so that should come at the end, as a conclusion, not as a beginning.

The way I see it, I would think that what Wiegner did is to say, "Yes, you run the experiment, you come up with a model, but that model is broader than merely explaining that one experiment. So in a way, it is justified to put the broad model at the beginning and show that this one experiment proves one case." I don't know how fundamental these differences are, maybe just a style of looking at things.

Maher: How would the model that you speak of differ from, say, the posing in a systematic way of a hypothesis?

Jenny: I would say a hypothesis would require more fundamental assumptions. These models are really elaborations of colloid chemical theory or atomic theory, not a fundamental change in one of the basic assumptions or axioms. So I give the word "hypothesis" a more profound meaning than many people do: I know that in the social sciences every little guess is named a hypothesis and they prove it and disprove it and all that. Well, I would reserve the word hypothesis for a more fundamental thing.

Maher: But, concerning Bradfield, this was enough that he spoke about it in his address?

Jenny: Yes, these were the things he discussed in the talk he gave. So I really don't know in the long run how much he got out of that stay abroad. This is difficult to assess. One may not even know about the value of such an experience for one's self. You may even delude yourself about how much it has affected you. So I don't know. At least, he appeared to understand more about the way I thought and the way I behaved and the problems I was facing.

Maher: Did Wiegner ever write to you subsequently alluding to the visit by Bradfield?

Jenny: No, he never wrote about that.

Maher: Did he write to you?

Jenny: Yes, he wrote once or twice a year. It surprises me now, because he was very busy, exceedingly busy. In fact, he was, I would say, busy from morning to noon to night. He was busy with all the people and with the administration of his institute. I don't think he had time to go to a concert to the theater or read, although once or twice a year he took a week's vacation, going to Italy or to some other place. He went to many meetings all over Europe. He always took a few days, so that was a relaxing time.

Maher: Do you still have those letters?

Jenny: Yes, I still have some letters around.

Bradfield's Personality and Background

Jenny: Bradfield is still alive.

Maher: What was he like at that time?

Jenny: He was young and vigorous, a real go-getter and domineering. He knew exactly what he wanted and he would go after it. If he had decided to run for president, he would run for president and would probably make it. At meetings, he would stand up and argue and discuss in his very strong voice and very positive manner. He made a strong impression on people. He also had an imposing physical figure and facial expression.

He has a son here in nutrition who has had many difficulties working with the extension service. He was fired from that job a few years ago. He was always in the campus paper, the Daily Cal.

Maher: Did that have to do with nutrition of the migrant workers?

Jenny: Yes, he was in nutrition. He was one of his six boys.

Maher: Is that man still around by any chance, do you know?

Jenny: No, I don't think so.

Maher: I meant in the state of California. It would be interesting to talk to him.

Jenny: Yes, I would think so. He has a home in Orinda.

Maher: Do you know his first name?

Jenny: His first name is Robert.

Maher: How did Bradfield's background and academic training compare with Wiegner's and with yours?

Jenny: I don't know for certain. He was very intelligent but I don't know his background. You see, what he was doing was new. He entered new territory. He didn't have to rely on old techniques. I would say he had a conservative chemist's point of view. At least, I never had any discussions with him or heard of him talking about modern developments in chemistry and physics. He was busy in the Masonic Lodge and pursued many other activities.

Maher: Given the two personalities that you now knew, how do you think that Wiegner and Bradfield matched up as people to work together?

Jenny: I would think they clashed and maybe Bradfield's trips to Vienna and Berlin had something to do with it. I don't know. I never gave it any thought until you mentioned it right now. But I would think they were both strong personalities and had strong ideas and they thought they were right.

Farmers, Academics, and Politics

Jenny: Bradfield was always described to me by my Missouri colleagues as an ideal scientist because he grew up on an American farm, whereas I didn't. I was criticized, or it was frequently mentioned, that I had the disadvantages of not having grown up on an American farm.

Maher: Mentioned by whom?

Jenny: Well, by [M.F.] Miller, the boss, or other people. [Leonard] Baver, who was very good, also had grown up on a farm in the Middle West. So apparently being a farmer was a big thing at Columbia, just to get to know all of the practical questions the staff talked about. I knew nothing about growing corn, which was the big thing, and, while I did farmwork in Switzerland--the tools were different. Everything was different. A new vocabulary and different worlds existed. I could not talk to people. Once Miller took me to the Kiwanis Club to give a talk. Well, that was all right--I could talk about my mathematical curves. But the moment it came to applied things, I didn't know enough about growing corn and raising hogs on the American scale. So I had a distinct handicap in translating my work to the practical farmer and at that time, in Missouri, that was important. You had to be able to talk to the farmers in their language. Sort of a cult was made of it.

Maher: Amongst academics?

Jenny: Yes.

Maher: Do you perceive that the university system in Missouri at that stage was at a period of growth where they needed to cultivate the confidence of the farmers. Is this something that has perhaps changed over the years?

Jenny: Well, I would think it had definitely changed, because they wouldn't have been able to keep a man like C.E. Marshall. (After I left, they got Marshall.) But the general trend with respect to the faculty at that time at Missouri was that the good people would leave and the average people would stay and gradually go into the administrative positions.

Maher: When you say "good people," you mean good by what criteria?

Jenny: Professionally good.

Maher: Academically good?

Jenny: Yes, academically good.

Maher: Did the fact that members of the faculty had grown up with a farm background have anything to do with the good people leaving?

Jenny: The reason they were called away had nothing to do with the farm background. Of course, Missouri, at that time, was a state rich in politics. The administrators and heads of departments were politically active. The Pendergast clan was operating in Missouri, and I think they were under a lot of pressure from practicing farmers and from the state legislative representatives to defend the university. We were pressured to publish or write bulletin articles with practical applications and I wrote the one on the prairie and the cultivated field and the effect of cultivation. That was a practical publication, a bulletin for farmers.

Maher: How do you think the farm population or people who valued a farm background and practicality responded to that paper? Did that help you gain confidence within their world?

Jenny: I don't know. I don't think a practicing farmer was really interested in it, or could read it, but the agricultural advisors, the extension service, teachers in ag schools or in high schools where they taught agriculture--these people would know about it. But I have no way of knowing whether this bulletin had any impact on practical agriculture. I don't know.

Jenny: Later, Bradfield played a decisive role in the work of the Rockefeller Foundation and he had lots of ideas about many things. I would say he was a very important figure in American agriculture and soil science, and he carried great weight. Whenever I had difficulties, as in having a manuscript accepted by Soil Science, the editors turned to him and he got it through. When my manuscript, Factors of Soil Formation, was turned down by McGraw-Hill, I took it up with him and he got it through. So he was an influential person.

Maher: What was the year of that first instance you speak of, the year the journal article was turned down by Soil Science?

Jenny: It was in 1935. Yes, that was lambasted left and right. The reviewers said it shouldn't be published. It was the paper on clay formation in relation to climate. But the people in Washington denounced it and said it shouldn't be published in Soil Science. It should be published perhaps in a journal on ceramics or something like that.

Maher: Maybe we will get a chance to speak about that later.

Jenny: That schism is still current, the cleavage between the bureau people and the others, including myself. It has to do--among other things--with the definition of parent material, whether the rock is the parent material or the weathered rock, and at what depths. I think later on we might talk about how Marbut and his people looked at soil. How they collected soil shows their whole philosophy. Marbut, at times, would collect only A and B horizons. He would not collect the C horizons because that wasn't the soil. That was the parent material. Such notions are still around.

Research with Bradfield on Soil Colloid Chemistry

Maher: Getting back to your being in Bradfield's lab, in the first year or two years, what was the lab like? How was it equipped? How did it strike you as to its equipment by comparison with the labs you had been accustomed to in Europe and in Waksman's lab?

Jenny: I would say it was excellent. It had a lot of new things that I hadn't known existed. At that time pH was still a big new thing. In fact, I think the first paper I ever wrote was on pH. But we had to make all our equipment. There wasn't any commercial equipment available for pH meters. Bradfield had a lot of material there, all the wiring, tools, and everything so you could put together an apparatus. So I learned a lot and it was very instructive.

Maher: So you were encountering equipment new to you, like the pH meters?

Jenny: No, the pH meter was not commercially available, but the facilities were there to make one.

Then there was the electrodialysis apparatus, the one you are interested in. That was new as was his whole system of electrodialysis cells. I did a lot of work on that, trying to understand it and to see what was happening. He had funds and it was a good lab located in the basement. It was rather dusty and, of course, I was not used to the hot summer weather in Missouri. We would work down there at night when it was still ninety degrees at 11:00 at night, with 100 percent humidity. I didn't like it.

Maher: So he had plenty of funds, but these funds were used primarily for purchasing equipment in order to make equipment?

Jenny: No, only in case of the pH meter. Apparently, he was very ingenious and very good with his hands. In fact, I had other experiences all over the United States where I encountered people who grew up on a farm and went to work in a lab. They were very good with their hands and didn't hesitate to try anything. Professor [Perry Robert] Stout was a classic example of that type of person.

Maher: Do you think this is part of what was meant by people who criticized others, like yourself, for not having a farm background?

Jenny: No, I had the European farm background. That wasn't it. They realized that I could do these things. It was more about talking in farm language. You see, it was important to them to have a fellow coming from the farm doing theoretical work. That was itself a justification for doing basic research. There was a connection, so the farmers couldn't complain if their own people wanted to do that kind of research.

Among American researchers in clay and humus chemistry, Robert Bradfield in Missouri and Sante Mattson of the U.S. Department of Agriculture (U.S.D.A.) in Washington, D.C., were the most prominent. Mattson correlated hydration, swelling and dye adsorption of clays with their silica-sesquioxide ratios and later generated amorphous floccules of clay, humus, and iron mixtures. Bradfield promulgated the clay-acid theory according to which acid soils were said to contain clay-acids that could be characterized by definite disassociation constants, similar to acetic acid. This notion was anathema to the adsorption theorists of the Wiegner school, who stressed diffusive ion swarms surrounding colloidal particles. In Zurich, my ion exchange experiments with artificial clays (permutites) revealed striking "lyotropic series," that is, the exchanging mono- and divalent ions could be lined up in series of increasing ionic radii. This behavior was considered typical of

Jenny: colloidal systems and had not been observed in true dilute solutions, such as those of acetic acid, clay acids, or of Donnan systems.

Applying the Jenny Thesis

Jenny: Upon Bradfield's return to Columbia, he and I concluded that a repetition of my thesis work with clay acids extracted from Putnam soil would tell us which view was correct. To my dismay, the lithium ion acted like the sodium ion, which it shouldn't have, according to its hydration. To Bradfield's discomfort, the potassium ion was a much better exchanger than the sodium ion, which also shouldn't have happened. Thus, nothing was settled. I extended the inquiry to all sorts of clays and soils. Without exception, potassium (K) was taken up more readily and held to the surfaces more strongly than sodium (Na). A digest of over 200 published soil profile analyses from all parts of the world permitted tying this K-Na principle to parent material and climate and pointed to its significance in geochemical cycling. The response to this effort, published as Bulletin 162 in 1931, was practically nil. As far as I am aware, geochemist V.M. Goldschmidt and soil physical chemist C.E. Marshall are the only ones who referred to it.

Further, the exchange studies with beidellitic Putnam clay exposed at high salt concentration the existence of an exchange maximum, which was contrary to Freundlich's adsorption equation, and which favored a Langmuir type chemical formula. I discussed these conflicts in a lengthy paper in the Journal of Physical Chemistry (1932) and, needless to say, they evoked in my mind skepticism about Wiegner's ideas and to some extent my own.

Examining Ion Exchange Processes with Natural Clays

Jenny: The time seemed ripe for examining ion exchange processes with natural clays on a broad basis. With a grant from the National Research Council, John Gieseking, on leave from Illinois, performed painstaking analyses and R.F. Reitemeier measured the flocculation values for the same clay suspensions. By conventional ultramicroscopic techniques, he measured the electrokinetic behavior (zeta potentials) of the clay particles. The combination of the varied observations furnished a fusion of ionic reactions with stabilities of clay suspensions. This was enlightening to me and to the many

Jenny: students at Columbia and later at Berkeley. The presence of Bradfield's student, L.D. Baver, of Guy D. Smith from Illinois, and later, of Hans Winterkorn, contributed to an envigorating atmosphere.

In spite of the good and steady flow of work, I felt frustrated because I was in the crossfire between Wiegner's and Bradfield's battle positions. For my own peace of mind, I needed a picture of colloid reactions that offered fewer conflicts. The Braggs [William Henry and William Lawrence] and [Linus] Pauling had made great strides in deciphering crystal chemistry and I could see that the adsorbed exchangeable ions function as charge-balancing counter ions of the crystal lattices. I visualized them as being mobile and oscillating to and fro on the solid surfaces. Their displacement by solution ions was merely a statistical redistribution. I figured that the ease with which the exchangeable ions are displaced depends on the average distance of hopping, which correlates with the thickness of the electric double layer and the magnitude of the zeta potential. To my great astonishment, the equation that came out of the mathematical hopper was a variant of the law of chemical mass action, the law that Wiegner rejected as being unsuited for colloids.

I was pleased that the exchange constant contained the ratio of the oscillation distances of the two participating ions. Although Gieseking's curves fit the equation nicely, I could see that actual clay systems would rarely, if ever, conform to the simplifying assumptions made for the model. This insight discouraged me from further pursuing exchange equations, the moreso, as my modest mathematical background would be inadequate for more ambitious modeling.

In fairness to Wiegner and his period, his objection to the mass law antedated the transformation of its concentrations to the activities of G.N. Lewis of Berkeley. I had used solution activities at Zurich but did not know how to handle activities of adsorbed ions. In 1932, A.P. Vanselow at Riverside took a stab at it, and my later colleagues, L.E. Davis, R. Overstreet, and K. Babcock advanced the thermodynamic formulations of ion exchange equations, a tradition that is still flourishing. However, the bridge to electrokinetic phenomena is missing.

In April 1936, the same month that the statistical model appeared in the Journal of Physical Chemistry, Professor Wiegner died at age 53. He must have turned over in his grave, as judged by successor Hans Pallmann's reaction to my papers conveyed to me via the grapevine. (Marbut had died the year before.)

Jenny: Two laboratory observations of future portent deserve recording. As mentioned in earlier interviews, during my thesis work in Zurich, I was curious about how plant roots avail themselves of exchangeable ions. At Columbia, Missouri, E.W. Cowan and I grew soybean plants in calcium (Ca)-clay suspensions and we reported in Science (1933) that Ca taken up by the plants matched the titratable acidity that appeared on the clay. Evidently, the roots had exchanged two hydrogen ions for each calcium ion taken up.

Regarding the second event, Eric Winters, from the University of Illinois, spent the spring of 1936 at Columbia studying movement of iron in soils. One day we decided to place ground ferrous iron minerals on acid clay gels in test tubes and we noticed downward diffusion of iron. Migration was more pronounced in thick gels than in thin ones, which is contrary to diffusion of ions in solution. It did not occur to me just then that the two experiments could be viewed as Siamese twins of a broader principle.

Hiatus: The University of California at Riverside, 1934 and 1935

Jenny: I am getting ahead of the chronicle and must retract a couple of years.

By now I was an assistant professor at $2300 a year. In 1931, my daughter Joanna was born and, in the same year, I was awarded the Nitrogen Research Award of the American Society of Agronomy.

Meanwhile, the stock market crash of 1929 initiated economic upheaval that engulfed the university. Young faculty members were laid off. I was spared, in spite of complaints that the College of Agriculture at the university favored a foreigner over native sons. Fortunately, the emerging Soil Conservation Service (SCS) absorbed many of the department's newly unemployed. The new adventurous work was organized by soil surveyor H.H. Bennett, and Missouri received special consideration, because years before, F.L. Duley and M.F. Miller had established the first systematic soil erosion experiments. The SCS was to occupy my time profoundly at a much later date.

In 1934 the depression was still going strong and Missouri faculty members were forced to take a year's leave of absence without pay. During the leave, some were even told not to come back. My turn was set for the year 1934-1935. With the aid of my old Zurich friend, Max Kleiber, then professor of animal husbandry at the University of California at Davis, I made contact with the Dean of Agriculture, Claude B. Hutchison, at the university's Berkeley campus. He knew of me from when he had served on the

Jenny: International Education Board and arranged for me to stay at Riverside, California, to work with Professor W.P. Kelley on theoretical soil chemistry and with station director, L.D. Batchelor, on applied soils work. For our family, it turned out to be a wonderful exciting year professionally--living among snow-covered mountains and fragrant orange groves. Riverside was then a neat, little town. I used to go horseback riding with Dr. Batchelor and we camped on mountains and bathed along the shore of the Pacific Ocean.

I was pleased to be assigned to W.P. Kelley, who had a great reputation as an arid lands soil scientist and as a successful reclaimer of alkali soils. In 1931, he and Dore discovered the crystalline nature of clays, which I had approached but fumbled on in Zurich. Once at Riverside, I soon found that it was not Kelley but Dore, at Berkeley, who had the ideas and the x-ray technological know-how.

Seemingly Kelley didn't know what to do with the discovery. Each morning I lectured to him on crystal chemistry. He was an attentive, keen listener and before long we engaged in fruitful collaboration in ion exchange studies with minerals. We published two papers in Soil Science in 1936.

In collaboration with E.R. Parker, my practical soils work dealt with penetration of irrigation water in orchard soils, as modified by soil management practices. Later, in our paper published in Soil Science in 1945, we became protagonists for non-cultivation of orchards, a method practiced earlier by orchardist F. Hinckley, and advocated by W.R. Schoonover, a University of California extension specialist in soils.

Halfway through the stay at Riverside Kelley urged me to visit Berkeley, where I delivered one or two seminars for Professor D.R. Hoagland's Department of Plant Nutrition. At the end of the year, I heard rumors that the university was trying to find a position for me.

Waksman, back in New Jersey, once remarked to me that America is an irresistible magnet and that one day I might become a citizen. "No chance," I laughed. Yet a decade later, in 1936, I was waiting with witnesses to be naturalized in Missouri's Boone County Circuit Court. The judge delayed appearing, and I said, "Maybe he is looking up some questions." Stadler quipped, "Maybe he is looking up the answers."

Later in the year, I was offered and accepted a position as associate professor in the Department of Plant Nutrition at U.C. Berkeley. I did not bargain with my Missouri friends. Bradfield had left for Ohio, Baver took his place, and I felt like the fifth wheel on the wagon.

More on Bradfield's Study of Putnam Clay

Maher: You have said that Bradfield was a rather "conservative" chemist. At the same time, you characterized his work as entering into a new field. Can you explain that?

Jenny: The new field was the understanding of clays.

Maher: Natural clays?

Jenny: Yes. He was involved with a practical clay problem, concerning a clay pan soil in Missouri, and the soil department was trying to make that land productive. That was the object of his study of clays.

Maher: Was that the Putnam clay?

Jenny: The Putnam clay and the B horizon of the Putnam clay soil type. But when I say that Bradfield was conservative, I would say conservative in the sense of classical chemical ideas, such as the simple form of the law of mass action without activities. His titration and conductometric curves were all of the standard forms. I'm sure he must have learned a lot under Wiegner because I felt that in succeeding years he was more open and accessible to many colloid chemical ideas, perhaps primarily as a result of Baver's thesis on the lyotropic series.

Maher: Let's go back to get a little bit more detail on the new fields that Bradfield was going into. You mentioned it was rooted in the fact that he was confronted with a clay pan problem, the Putnam clay, in Missouri. Can you expand on that and show how things developed in terms of ideas and experimental work?

Jenny: I don't know whether I know the story. There was and still is this tight clay pan. The roots couldn't go through. So he took a sample into the lab, developed methods to disperse it by churning and making suspensions, and then he applied centrifuging suspensions to get finer and finer particles.

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Maher: Did you say you were sure those were techniques of isolating the clay that he hadn't learned in school?

Jenny: Yes, techniques of dispersing the soil and, I think, adding dispersing agents. Then he had a suspension and he centrifuged it with the super-centrifuge. I don't know how he got hold of these ideas and all that. He must have read about centrifuges, or he had a good nose with which to find things.

Maher: But this is how he got involved in clay.

Jenny: That's the way he got into clays. He did not come, as I did, from a theoretical background. I was taught the theories of clays first. He came the other way. He worked with his hands with these clays and arrived at ideas.

Maher: How did this line of activity and pursuit transform into Bradfield's interest in acid clays, or did it?

Jenny: He found out, of course, that these Putnam soil clays were acid and so he advanced the chemical idea of clay acids that neutralize bases have dissociation and hydrolysis constants, and he determined these properties. Of course, the thing that I was brought up with was the adsorption phenomena of clays.

Maher: Physical adsorption?

Jenny: Well, or chemical, not necessarily physical. But it was a different thing. In Wiegner's thinking, you always started with forming a colloidal particle that would adsorb OH minus and H plus ions from water and then develop the electric charge and ion exchange. But in Bradfield's theory about clays, the charges were there to begin with. They were always there. You didn't have to introduce them. I liked that. The more so, since there had been new theories in colloid science for silver salts (silver chloride), where the charging ions were related to the lattice of the colloid particles. So clay acid was really a simple extensions, although Bradfield didn't look at it that way. I think Bradfield's work cultivated or strengthened the idea in my mind. When I developed an ion exchange model, that was it; the charge was given. It was a property of the lattice of the colloidal particle.

Maher: Could you clarify the transition point of the model of silver chloride and the lattice of a silver chloride precipitate?

Jenny: The model was very specific. I think the ideas go back to [Casimir] Fajans. They were the result of the discovery of the crystallinity of colloidal particles, or they had been developed independently. Anyhow, the picture was that you had this silver chloride lattice, and you had silver ions and chloride ions exposed on the lattice surface. They might dissociate, become exchangeable and impose a charge on the particle in relation to pH. For acid pH you would have one kind of charge. In alkaline pH another kind, similar to what had been known for a long time about proteins.

Maher: But wouldn't this just be a surface energy phenomenon as is true of any crystal?

Jenny: Not any crystal, because at that time it was not believed that, say, quartz had this phenomenon. Quartz was not considered to have ion exchange properties. What you are postulating is a much later development. Today, we look back and we don't understand how Wiegner could have missed such a thing.

Maher: Yes. We want to be really careful about that. I'm having a good deal of difficulty separating this in my mind. Actually, since we are speaking about this matter of crystallinity and trying to isolate where ideas came from and at what point they were changing, I have had a question which I wanted to pose to you and I might as well do it now. It struck me when I was reading over the history of colloid chemistry and read about Thomas Graham's early work, that I found that, with a terminology of his own, he separated the world, so to speak, between what he called colloids and crystalloids.

Jenny: Yes.

Maher: Now, when I saw that, I thought, "Gee, that really may cause people to think in a pre-judging sort of way from long usage of the language that a colloid is not crystalline."

Jenny: That's right. That was the prevailing idea and I remember that Wiegner taught about Graham's ideas using these very words, "crystalloid" and "colloid," as he wrote on the blackboard.

Maher: When he used the word "crystalloid" as opposed to "colloid," was that describing a colloid as not being crystal?

Jenny: Very definitely, a colloidal particle was not considered to be crystalline. That was the basic tenet. If chemists thought of a sodium chloride crystal and if they had logically developed a model, they would have concluded that there must be smaller and smaller sodium chloride crystals down to colloidal sized particles, which would be crystalline. But I never heard that thought developed. Anyhow, when I took the early courses, just like a student nowadays, I had to get it done with and I couldn't afford to think it through and contemplate. Like students now, I just accepted it. The thinking was, "Here is a great man; therefore you accept." Theoretically, the more you accept everything, the farther you are ahead, which of course isn't always so. It could set you back.

Maher: I have another small question that goes back to Wiegner. Again it has to do with the growing understanding of the colloid particle. The Brownian motion that had been observed under the ultramicroscope evidently could be accounted for in two basic facets, one being translational motion and the other being rotational motion. If the particle were a sphere, then the rotational aspect would not be showing up whereas if it were odd-shaped, there would be an increase motion from the rotational aspect and it has been stated as

Maher: perceivable, physically perceivable, by changes in color under the ultramicroscope. I was curious whether you and Wiegner observed that.

Jenny: Oh, yes. You see, if the particles are rods, like a pencil, for the same number of particles you count fewer particles, because every so often the light hits only the heads of oriented pencil particles and they don't show up. So, no, that was very clear and Wiegner published on that thing. I think a fellow from England, the son of the famous Englishman, Sir John Russell, director of Rothamsted Experiment Station, who was on the extended field trip discussed earlier worked with Wiegner on that very thing.

Maher: About what year would that have been?

Jenny: That was after I left. Russell was there in the early thirties.

Maher: That conforms with my reading of the paper that you gave me this morning, the one by Wiegner. In it, he discussed what he considered to be a fairly new theory on the non-symmetric, non-spherical structure of many particles and in that article he explicitly mentions the platelike structure of clays. It is apparent, as the article continues, that he even refers to the clays by then as crystalline. So this paper was written at a turning point. He had evidently gone over the hump of recognizing and speaking of clays as crystalline in structure.

So now, what I am trying to zero in on is this: At what point was he observing--and perhaps you had also observed this in the lab earlier back in Switzerland--at what point were you and Wiegner observing this asymmetric rotational behavior of some of the particles? You would have had to be looking at natural plates.

Jenny: That was in full swing when I got there. Wiegner had been working for years on what is known as vanadium pentoxide. Those are long rods and he worked with those. They flocculate readily. No, he was quite aware of all that.

Maher: That was pencil-like in structure?

Jenny: Yes.

Maher: How about platelike structures?

Jenny: Well, platelike structures were known for iron oxides. Freundlich's lab had developed that research. They are not as simple structures as the spheres or rods because you never get a zero diffraction and, even if the plates are horizontal, they are still aligned so they don't vanish like the rods. So you would not see a difference in the number of particles if you count them like you do with the rods.

Maher: I'm having trouble separating my strong reaction to the image of a clay particle being platelike. To me that would immediately conjure up the possibility of a crystalline structure and I would like to know whether there was any discussion of such a conjecture at the time? Was it observed as platelike prior to Wiegner's, or anybody's, ever having applied x-ray crystallography?

Jenny: There were theories in colloid optics, and with them you get colloid optical effects, like the diffraction pattern or the Tyndall beam. A plate doesn't have to be crystalline or a rod doesn't have to be crystalline. For plant tissues, my friend Frey-Wyssling, developed the experimental criteria to distinguish whether a plate is crystalline or not. So what you conjectured wasn't necessary. You could have an amorphous rod. Now that this has come up, I wonder how an amorphous rod would work. I would think if a spider net collapses the pieces would be rods and not necessarily crystalline.

Maher: And plates?

Jenny: No, not plates but the amorphous rods would be more of a string or like a thread in a spider web.

Maher: Could you give an example of a platelike structure that was not crystalline at that size?

Jenny: Do you mean how it could work?

Maher: Yes.

Jenny: I could see that if you, for example, precipitate aluminosilicate, and it is squeezed between other platy crystals, it would be a plate, and it would be amorphous. So it depends on the environment in which the precipitate is formed. Especially, if it forms near a rod crystal or between loosened mica-plates, it could be amorphous, initially.

Maher: Do you recall any discussions of this nature ever having taken place?

Jenny: In Wiegner's lab?

Maher: Yes.

Jenny: No. [pause] But he might have talked with other people, like [Herman] Gessner. Gessner got his Ph.D. with his work on vanadium pentoxide. They might have discussed it. I don't know. We'd have to look back.

Maher: I have one more thing that I want to ask before we move on with Bradfield's research on acid clay. Do you happen to know whether it was in the mid-twenties when Bradfield began his work on the acid clays?

Jenny: Yes. It was in the mid-twenties.

Maher: Do you happen to know whether he had any inkling of investigations in Japan around 1912 and later, with respect to acid clays?

Jenny: I don't know, but of course, at least in Europe, we knew about [G.] Daikuhara's work. It was well known. But that is for acid soils, not acid clays.

Maher: Yes, I understood you were speaking specifically of acid clay.

Jenny: I don't know. You see, in Europe the soils are really not rich in clay. The few soils that are rich in clay are used for commercial purposes, but agricultural lands and forestry lands don't have much clay compared to what we have in the United States or in the tropics. So I could see that Daikuhara might have had a clay soil that was largely clay, so he had an acid clay. No, I don't know how Bradfield got to that. You would have to ask him. It would be interesting to do that. The next time Bradfield comes here you should ask him about that.

Maher: Oh. Do you think he will come?

Jenny: He comes every so often. I see him every few years. He visits his son. You could find out when he is expected to show up. When did I see him last? It wasn't many years ago.

Building on Bradfield's Work: Research on Clay Colloids

Maher: Okay, now why don't you tell me how, with Bradfield's work as a background, your work developed on clay and soil colloids and cation exchange and all of those related phenomena?

Jenny: The thing that struck me about the acid clay was that in Zurich, in my ion exchange work, I prepared acid clays by leaching with dilute hydrochloric acid (HCl). But they were acidified permutites and they were not stable; they collapsed, and reacted but little with potassium chloride (KCl). I really had no experience with real acid clays.

I had always wondered about acid clay. I read about an acid clay that was stable in a paper by Van der Spek in Holland. It was written in Dutch. At that time, in the early 1920s, I was in Paris,

Jenny: and Wiegner sent me a copy of that paper and wrote, "Please translate this for me." Well, I had never learned the Dutch language, so I had to get a dictionary and work on it. Van der Spek reported: Yes, there were clays and they were acid; they had hydrogen ions. Wiegner realized that that was an important paper and it was contrary to what I had found with the acid permutites. I remember I added KCl and hardly anything happened. Nowadays we know a clay makes a real acid. So that was the first thing and then in Bradfield's lab I worked with these clays. I don't know whether Bradfield was influenced by Van der Spek.

In Bradfield's lab, I saw that Missouri clays were strongly acid against litmus paper, which turned deep red. Right then, I started with Albrecht, to study the effect of the degree of calcium saturation on the growth of soybean plants. That opened a new way of doing things.

Maher: Did Bradfield by any chance have any Japanese references in his paper?

Jenny: I don't remember. I was not really interested in how Bradfield arrived at his ideas. I was frustrated by all of these conflicts that came up and I was always on the defensive. Naturally, the graduate students simply accepted Bradfield's ideas and I had difficulty refuting them because I was still tied to the permutites.

Maher: So, in the experimenting, how did your thinking proceed?

Jenny: While Bradfield was gone, I did not develop any colloid chemical ideas or experiments. I was merely learning his techniques, and I made calcium-hydrogen clays while working with Albrecht. When Bradfield came back, we discussed the conceptual differences, the schism between Wiegner and Bradfield, and then concluded I should run ion exchange experiments that should show who was right.

It took me a long time because at first I compared lithium and sodium and their exchanges came out nearly the same. I assumed that there must be an impurity of calcium (Ca) or beryllium (Be), in the Lithium (Li) salt. So I spent a lot of time analyzing lithium salts for all sorts of contaminations. The analytical methods were still cumbersome and slow, so I spent almost a year convincing myself that Li behaved like Na in Putnam clay, because in permutites the two ions exchanged with different intensities.

Then I was curious as to why that is so I studied several kinds of clays. I could show that the lyotropic series depended on the nature of the clay, and that it might be explained by the hydration theory of ions: that highly hydrated clays may dehydrate an ion. That was new to me, but it made sense that the exchangeable ions were not as fully hydrated as in dilute solutions. On the surface you have reactions that do not occur in dilute solutions and you

Jenny: can't use the activity co-efficients of dilute solutions. But it took decades for other people to unravel the activities of adsorbed ions.

It was, in a way, a disturbing period. [pause] Because of these ion discrepancies, I started to re-investigate clay behavior, such as flocculation values and migration velocities, and all of these things formed a consistent picture.

Maher: Can you describe that consistent picture?

Jenny: Well, this whole phenomenon with the hydrated ions was called the lyotropic series and investigators still use this term. It turned out that the lyotropic series varied from soil system to soil system. But within any one of the systems, if the various ions have a certain sequence in the lyotropic series of exchange reactions, it is reflected in the migration velocities in an electric field; it is also reflected in the flocculation values, and in viscosities. Thus, if you assume a certain degree of ion hydration in a given system, then the ions act that way, whatever the systems does, and in that way the picture is consistent.

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Maher: You were saying that you didn't talk this through with Bradfield because he left not long after he returned to Missouri. Did all of this take place in about a year?

Jenny: A year or two, but I would have to look up to see when he left for Ohio.

A Statistical Model of Cation Exchange

Maher: Could you take us ahead to the paper you published in 1936 on the statistical view of the mechanism for a cation exchange? In terms of your thinking and work at Missouri, is that connected?

Jenny: That work was done at Missouri, and because of my ion exchange findings and because of the conflict between Wiegner's ideas and Bradfield's ideas, I tried for my own understanding of how ion exchange occurs, and whether one could derive an equation according to a mechanistic model, not merely applying the law of mass action or a thermodynamic equation that really doesn't explain anything. I felt the need of wanting to understand what these exchanging ions are doing. So I set up a model of a mica-type clay plate, and visualized that ions on the surface are not sitting still but they oscillate back and forth. There were a lot of questions I couldn't answer, such as: "Just how far out does the double layer go? How

Jenny: far out does the electric field extend? What, except Brownian movement of water molecules, forces an ion out, and what forces an ion in?" I had no clear picture until I developed the statistical action model. At times an ion might oscillate across the whole distance of the double layer. Meanwhile, a wandering solution ion comes in between and oscillates there while the other wandering ion is left out. To me, this made a plausible picture. It was the first statistical model, as stated, by Friedrich Helfferich in his book (1962).

Maher: What was the experimental basis that you were using for the distance of oscillation that you were conjecturing?

Jenny: I did not have to assume an absolute distance, because if you have, say, an ammonium clay and a potassium clay, each kind of ion has a different oscillation distance and what finally enters into the equation is the ratio of the two distances. That gives the constant of the reaction.

I used the statistical Poisson series. In terms of strict physical chemistry, what I did was perhaps not according to Hoyle, but I thought I would not need the Boltzmann equations. I felt that ion exchange was merely a statistical re-arrangement and that that was the main thing. It took me many weeks. I made a lot of errors and ran into blind alleys. I was working in the basement in the hot Missouri summer. I was astonished that the equation which showed up was the law of mass action, though not in the regular form: This I found really fascinating and like an illumination then. Moreover, I was pleased that I had derived a law of mass action result.

Maher: Do you mean you hadn't just accepted the application?

Jenny: No, I derived the whole thing, so I was quite pleased. Now I understood the whole ion exchange thing, how it comes about. Now, I knew also under what conditions the law of mass action applied and I could also see right away that Mitscherlich's law of plant growth was the law of mass action. Of course, I knew that Langmuir's equation was the law of mass action. They are all the same law expressed in different quantities--negatives, or differences, and all that. It was a fascinating experience.

There was one shortcoming which did not help the acceptance of the model and that was simply the kind of drawing I published about the surface. The oscillation volumes are sort of half spherelike shapes, but I couldn't draw them in perspective and we didn't have technical artists. So I drew them as cubes, and I know people would say, "Well, this is crazy; oscillation doesn't go in cubes"--although I had stated the reason. Now, looking back, what I should have done was to go to St. Louis and find a science artist and pay him to draw these half-spheres showing them in perspective. But I didn't. That was a mistake.

Mathematical Limits: Work on Exchange Ions

Maher: You characterized this, when you discussed this paper in your own write-up on the Missouri chapter. It seems as though you now consider this paper, this work, as a culmination of what you were working up to, and thereafter you were limited by mathematics. Could you explain when you reached that realization or how it was limiting you, if I am correct?

Jenny: Yes, the equation I derived was for exchange ions of equal charges: monovalent against monovalent; divalent against divalent; calcium against magnesium; potassium against sodium. But soon I wanted to derive an equation for calcium and sodium, and there I had to assume certain models and the simple statistical Poisson distribution, which I used, could not handle that. So I couldn't do it. But then my later colleague, L.E. Davis, who, as I said earlier, was a professor of soil chemistry at U.C. Davis, derived an equation for divalent versus monovalent ions. However, he did not get much further than what I did. Later, it was Overstreet and Krishnamoorthy who developed that further, using a thermodynamic approach. Their idea was simply to get a constant. This was their big aim, to get a constant for various combinations of ions at various concentrations. That was considered the proof that you had solved the exchange problem.

But I was never fully interested in the thermodynamic approach, because it didn't tell me how the ions really behaved. It didn't tell me any mechanisms. For that, you would have to go into statistical thermodynamics and there I lacked the mathematical background. Why I didn't dig into it, I don't remember.

Maher: At what time did you recognize that you needed the statistical thermodynamics?

Jenny: I noticed it in Missouri, at the end of the Missouri period when thinking about monovalent-divalent ion exchange.

Maher: By this time, how were you dividing your time in your work during your stay at Missouri?

Jenny: The emphasis shifted. Naturally, when I worked on equations, that was about all I did. I would say I had two things going: pedology and soil colloid chemistry. I developed both branches and I always wondered: Am I doing the right thing? Should I give up one or the other? I don't know what would have happened, had I done it. Both avenues proved very interesting and fruitful to me, so why should I give up either one? I could have two sweethearts. [chuckles] So I stayed with them.

Maher: Did they prove mutually fertile? Did the one stimulate your thinking and work in the other?

Jenny: Not in the sense that I came up with a new theory or hypothesis.

Geochemistry and the Potassium Sodium Ratio

Jenny: After I was convinced that potassium and sodium behaved differently, I went into what is now known as geochemistry. The behavior of sodium and potassium in the whole earth system, sodium accumulating in the ocean and potassium accumulating in the clays. So in that way I could see the combination of both fields, which the pure geochemist wouldn't see, or the pure colloid man could not see--that these two things hang together. That sodium accumulates in the sea was known for a long, long time; but--that it could be visualized going back to weathering and soil formation--that was new as far as I was concerned. I hadn't known about it.

Maher: That concept would have been part of the big, thick paper you wrote.

Jenny: I wrote a bulletin about it, Bulletin 162. Geochemists--the chemical geologists--wouldn't know about the soils work. So, in a roundabout way they came to similar conclusions by themselves.

Maher: Do you think that if that paper had been responded to more vigorously, that that would have become a third sweetheart?

Jenny: I don't know. What I should have done was to publish a summary paper in a geological journal and not just leave it as a bulletin. But what geochemist reads the bulletins from the Missouri Agricultural Experiment Station? No one.

For example, I read Goldschmidt, a very famous Norwegian geochemist. He wrote about potassium and sodium and the soils. So I sent him a copy of that bulletin and he wrote back. He liked it and then later, he mentioned it. But he wouldn't have heard about it. I don't know why I didn't do more. Well, for one thing, I did not have any plan for self-service or trying to push myself. I probably missed a good opportunity.

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Maher: Before we go into the Riverside period, I would like to discuss with you a little bit more about your potassium and sodium paper. I want you to give the explanation of how this arose and how you feel it has been dealt with since it's publication and that sort of thing. Let's begin with your background to this work and how your ideas evolved.

Jenny: All right, as you know, you don't sit at the table and come up with an idea. [laughter] So, as I mentioned last time, I think, there was this conflict between Bradfield and Wiegner, and I was in the middle. The Bradfield idea was that you shouldn't have had any lyotropic series, and in the Wiegner school that was an all important thing. When I studied the lyotropic series with the Putnam clay, it turned out that they were not as beautiful and simple as in the permutites. But what set the pattern, what alarmed Bradfield, was the separate behavior of potassium and sodium in the exchange reactions. Having found this with Putnam clay, Bradfield felt it was an unusual thing, and for me it was the normal thing.

Maher: Let me get something clear at this point. From Bradfield's view it shouldn't have mattered--if the clays were a weak acid, then it shouldn't have mattered because the disassociation constant of the clay, in his mind, would be the ruling factor, and the potassium or sodium would be--

Jenny: That's right. It's just as if you titrate acetic acid which is a weak acid. You can use either sodium hydroxide or potassium hydroxide and you would get the same answer. Okay, now I was curious to see whether the K-Na pattern was broader. I used other soils and analyzed potassium and sodium exchange, and accumulated a broad range of soils, including peat material. I had thought in Zurich that it must be a general thing, but that was merely an assumption, and through my association with Bradfield, I realized these assumptions had to be really tested, which I did.

Then I thought, "Is this a colloid chemical principle operating in soils on a world scale, and could we say it operates in soil genesis? I collected all of the exchangeable ion analyses from the literature. There were not many, because in those days the determination of exchangeable sodium was very difficult and not very accurate. But total chemical analyses of rocks and soils were available and I collected I don't know how many. They are all in Bulletin 162. If these K-Na exchange reactions govern soil formation, then in the soil potassium must accumulate and sodium must diminish, relative to the original material.

When I calculated the potassium-sodium ratio in the parent rock and in the soil, there was always more potassium in relation to sodium in the soil than in the rock. It was especially obvious if you look at the C horizon and compare it to that horizon which is the most leached horizon, which may be the A horizon or B. So that was clear. At that time, before I came to Berkeley, I didn't know about potassium fixation in clays, and I wasn't aware that plants prefer potassium to sodium. Therefore, the K-Na exchange principle is reinforced by these selections.

Maher: That makes it a little more clear to me. I was having difficulty understanding how the accumulation of potassium was being achieved through the mechanism that you're speaking about, because the capacity of the exchange sites themselves to retain potassium, or anything, is very limited in contrast with the total mass of the material. In other words, you could easily store 100 percent potassium on the exchange sites, but that would be almost negligible in respect to the total atomic make-up of the soil material.

Jenny: That's right. However, the emphasis was on comparing potassium to sodium. The moment these ions encounter a solution, sodium is more hydrated and the separation must occur, and that is still so. [pause] Therefore, it must follow that in the water, in the soil solution, the sodium must dominate the potassium relative to the solid phase--and that proposition goes to drainage waters, and goes to rivers and to lakes, and goes to the sea. So that established the trend of the ratio from the parent material to the soil, and to the ocean. So I became interested in the salt content of the ocean, its potassium/sodium ratio. That, of course, goes into oceanography.

Actually, what I was doing is what is now called geochemistry, but to me it was just an extension of soil chemistry. It has become a field of study that was started by a fellow by the name of Goldschmidt in Norway. He was a geologist, but also worked on the ionic radii (the size of ions), a very great, famous man. He wrote about potassium and sodium the same way I did, and I sent him my bulletin. He wrote me a nice letter, and referred to it later that this K-Na theme had been explored. He had thought he was the first to point all that out.

eaher: When did you send the paper to him?

Jenny: It must have been in Berkeley.

Maher: It was by the time you were here?

Jenny: Yes, I think so.

Maher: So the publication was in 1931 and then it was some five or six years later.

Jenny: It was later. I still have his paper someplace, where he referred to it.

Maher: When had his publication been accomplished?

Jenny: When I read about it?

Maher: Yes.

Jenny: Well, before that, but I don't remember what date.

Maher: But it was after this 1931 publication.

Jenny: Yes, I think so. Anyhow, it was done quite independently, I am sure of that. The K-Na problem is still alive, and the question is: is all that sodium in the sea derived from weathering of the igneous rocks? That's unsolved, because many claim that volcanic eruptions are a source. Then, in the ocean we have to account for the chlorides. Where did they come from, because from the soil these cations move out as hydroxides or bicarbonates, or maybe nitrates, and that's not what is in the ocean. So that's unsolved. Now [Issac] Barshad accepts that all the sodium in the sea is from the weathering of rocks, and he goes backwards and calculates how much rock weathering there has been and how much soil erosion there has been. That's the way he goes about it. I might add, a few years ago Barshad wrote a paper on sodium and potassium in the ocean. I used to discuss the topic in class. Also it made it clear to me that this potassium/sodium ratio would be influenced by the other ions.

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Maher: Before we leave this paper, can you explain how it fit into the gradually emerging components of what later will be your book? In other words, you had been dealing with carbon and nitrogen in the soil, that they related to climate. This was beginning in the Missouri period. You had begun to be aware of the necessity of chronosequences, holding everything constant at but one factor. There were other components to your book on soil formation. Was this study also destined to fit into that large scheme?

Jenny: It fit in in some ways. However, the K/Na ratio specifically deals with the mechanism, whereas in the book, Factors of Soil Formation, I did not discuss mechanisms, only the phenomenological functions. I have it only in terms of the ratio of K and Na relative to alumina, and climate.

Maher: So this pretty much stands as a tour de force, doesn't it?

Jenny: Yes, that's right.

The "Strum und Drang" (Storm and Stress) Period and the Importance of Dialogue

Maher: This whole Missouri period you once characterized as the "Sturm und Drang" era.

Jenny: Yes, that's a famous expression in German literature. Yes, that's what it was, because it disappeared when I came to California, or it showed up in a different way. Yes, it was a "Sturm und Drang" period because I developed these ideas about the state variables and the ion exchange reactions, contact effects, and all that, whereas the work at Berkeley was really an extension of those ideas--brought up to date, clarified, and intensified. But they were not related to an inner conflict which I had at Missouri. I was really torn to pieces then by what I was doing, where I was going.

Maher: Could you describe that more fully, that conflict?

Jenny: Sometimes you have nightmares, you wonder what you are doing, and about the future. Is what you are doing all wrong? People were questioning things like the nitrogen climate functions. They said, "Well, yes, it could be different but I don't see how." You really don't know. The conflict was that, while I was being accepted--I could see that people were accepting these things--I had the feeling that they really didn't understand it. It was merely aesthetically pleasing then or helping them with their ideas or something. But to me, mere acceptance was no support for my ideas. So all that Missouri period was a great uncertainty.

Maher: Do you feel that you missed out by not having a thorough enough dialogue with people?

Jenny: I think I had all the dialogues I could get, although I never had as good dialogues as I had with Overstreet here, because we worked closely together. With my friends at Missouri, we didn't work together. For a while, I had a dialogue with Baver and Winterkorn, but here at Berkeley it was better, because Overstreet had great competence, independently from his thermodynamics background. I had good dialogues with Hoagland, who was an authority on plant nutrition, whereas Albrecht was not. When Albrecht approved of data or ideas I had, I could not be sure that he really understood it. He accepted because he believed in me, or something. Actually, I guess my occasional doubts must have been the basic reason that, over the years, I tried to get more data on climate functions to get needed reassurance that this thing is not just a local situation by coincidence, but is a broad principle. So I went to the tropics, and I induced Harradine and Klemmedson to work up and down the Sierra.

The best people are the ones who will work with you and do some independent thinking and challenge you, but you don't find many of those.

On Art and the Natural Landscape

[Interview 7: May 24, 1980]##

Jenny: I would like to dwell a little bit about an aspect of the Missouri period and the Riverside period which we haven't discussed. For that I have to back up and show you some of the things I did in Switzerland. Is this recording now?

Maher: Yes, it is.

Jenny: I don't know whether it should.

Maher: Okay, I'll turn it off.

Jenny: Turn it off and if you want, we can resume it. [tape interruption]

It is an aspect--I don't think it has anything to do with science--but it will explain in part my great attraction for Riverside and California. I mentioned my friend Frey-Wyssling, a great botanist. We did a lot of mountain climbing and a lot of skiing together, and one summer we undertook a three-week hiking tour into the Alps. We kept a diary, and here it is. Now, Frey-Wyssling--or Frey at that time--was sort of a poet, and he wrote the text of the diary. I have done all the drawings and the photographs. I want to show you this record as an introduction to what I saw in the United States--my "art work," the sketchings.

Showing you the pages, I don't know how it will work out on tape. Anyhow, this is the title page and it says that it's the chronicle of two guys. This page is written in medieval German and in Gothic lettering.

Maher: Did you do that?

Jenny: Yes, I wrote that, wording and lettering. Here are we two. Here is Frey-Wyssling and here I am on that tour. Here is a map of all the hiking we did. Of course, as students we had no money. So Frey carried a violin, and I a guitar. Every evening we went to a village and in the village square we played music. People came around to listen to us. After a while we said we needed lodging. One or two would say, "We have a barn and hay and you can sleep in the hay"; and that's the way we worked for shelters at night.

Maher: Did you do this?

Jenny: Yes.

Maher: That's fabulous.

Jenny: It's not necessary to go through all of the diary. He was writing, I made the sketches. At Zurich, you see, I overslept on the first day! [laughter] Everyday we hiked for many hours, and we got closer to the high mountains.

Maher: It looks like you were joined by people.

Jenny: Here we passed this castle which was important in history. I made this sketch while we were having lunch. Then here are Swiss mountain homes. We went there to see our Zurich girl friends who were vacationing.

Maher: Is this Frey-Wyssling on the violin?

Jenny: Yes, that's right.

Maher: Then this would be you with the guitar.

Jenny: In one of the houses we had dancing and a game of "Schnitzelbank," and here are the sketches that go with it. Here are the girls we visited. Here is a photograph of myself with the climbing rope, the guitar, the glacier pick, and the crampons. Here is an alpine hut where we ate lunch. This is the way the people keep the milk, all in wooden containers. Then here we have a sheet of food analyses. We knew what milk contains and we calculated the calories so we knew how much we had to eat to climb a certain mountain! [laughter] [continues through pages] Here is an old home in Engadin valley. Here we had a picnic with pretty girls from Zurich. Here is the mountain peak we climbed. Here is another one. Here you see us climbing on the glacier with the rope. Here is the house in Fex valley we stayed at for several days with friends who had rented it for the summertime.

Maher: It's a big house.

Jenny: Yes, it's a nice, all-stone house, an old alpine farmhouse. I also sketched the inside, the kitchen. In these buckets you carry the water in from the creek. And here are all the pots and pans that go with the kitchen.

Maher: La Cuisine Fex.

Jenny: That's the place; that's the Fex valley. Our friends rented it and we were their guests for a week. They were French Swiss, so we spoke French. Here is the Edelweiss flower. Here is one of Frey's poems. Here is an old teacher (Sprecher) with Frey. Here I drew the little church in the same valley. That photo is interesting. It's the front of Fex glacier where the icy water comes out. We got ice and made ice cream. Here we looked down on two glaciers. See how they flow together and how they make the middle moraine? These are various photographs of our glacier climbing expedition.

Maher: Who had the camera?

Jenny: I had the camera.

Maher: What kind was it?

Jenny: Oh, it was just a Swiss camera. So you learn a lot of geology just by hiking around.

So what this diary shows is the interest I had in landscapes, and in drawing. When I came to New Jersey and to the Middle West, the motifs that I was used to drawing--mountains and castles and broad views--were not around. So the first--the only thing I could do--the first picture I drew when I came to the United States was this.

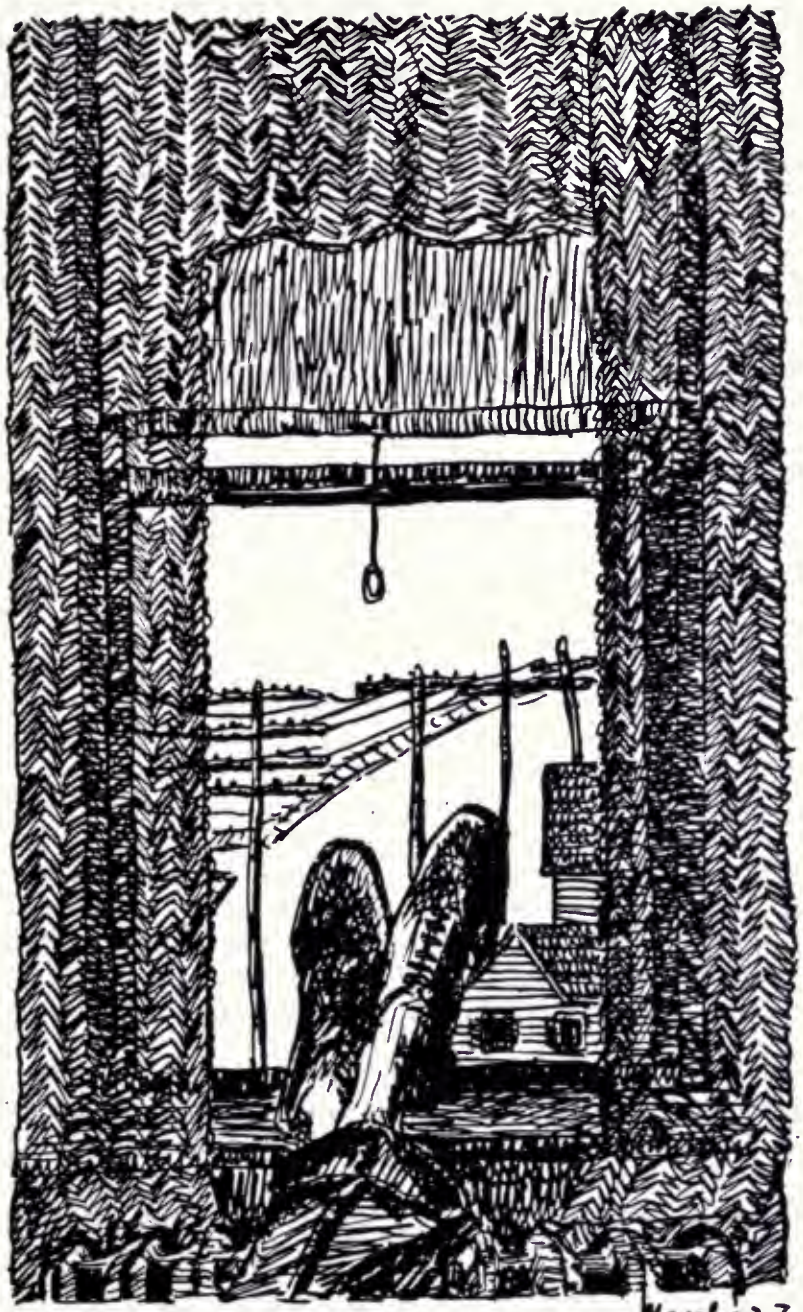
Maher: [laughs] A landscape in the U.S.A.!

Jenny: What surprised me right away was that everybody put their feet on the table or another thing, so I drew that, too, and these crooked telephone poles they had. So that's my first impression.

Before that I was in Italy. I had vacations in Italy, but that was all missing. Then, of course, I was impressed by New York in the United States. Here I have even some notes about profiles. Here--this is Knoxville, Tennessee. I know that's a professor from Romania. Here is a negro--black people. I've forgotten now who this was; maybe Wiegner. Then here is Kansas. I was impressed by Kansas, the enormous sky. So I tried to do that. I found this very difficult.

The only successful painting or recording of this enormous landscape of the Middle West, first was in Missouri; they had a painter by the name of Thomas Benton. He painted landscapes, but only certain segments of it. The real landscape painter was Grant Wood, from Iowa, and he drew the Iowa landscape, and especially he had a picture which I show in my art lecture. It's called "Arbor Day." It showed the rolling landscape, which no photograph could encompass, and he drew this and he succeeded in showing this <u>enormous</u> vista which you have in the Middle West, an enormous sky.

In Switzerland we don't have an enormous sky unless you go high up into the Alps. That's one reason we always went mountain climbing, to see the vista around. Otherwise, you're confined, mountains left and right, and you are sitting in the valley and see nothing.



Landscape in U.S.A. March 27

Jenny: So this was to me enormous, this whole Midwest. What I noticed was that the people and the artists, except for Grant Wood, didn't seem to be aware of the beauty of this landscape. Now, in Europe you have a lot of landscape painting. You have a lot of poetry and writing about the seasons, the snow season and the blossoms in spring, and all that. I encountered nothing of the sort in Missouri. I didn't know why that was. But I could understand then already why they really had so many abstract artists in the United States. Perhaps because this romantic and powerful landscape scenery was missing.

So when I came to Riverside this was a great joy to me. There were the mountains, the snowcapped mountains, and also the artists who painted them. There were always art exhibits wherever you went. You could see desert scenes painted, or ocean scenes, mountain peaks, and whatnot. I was back in the element I had grown up in. So this to me was an enormous attraction. Because actually, before I came to California I had about decided to go back and we didn't know what job I would get, if any, but we thought we would go back and then return to California until I got that job offer--which I mentioned earlier.

Maher: Was it after you had come here to California or after you got that job offer that you became a naturalized citizen in Missouri? I know it was in Missouri.

Jenny: Yes, after I knew there was a possibility of California. California to me was a big thing and it still is. Here are sketches; my wife was a musician so we had a lot of quartets. Here is a drawing of a scene along the Missouri River. It's just like a notebook. Here is a view of the Missouri River with the cliffs.

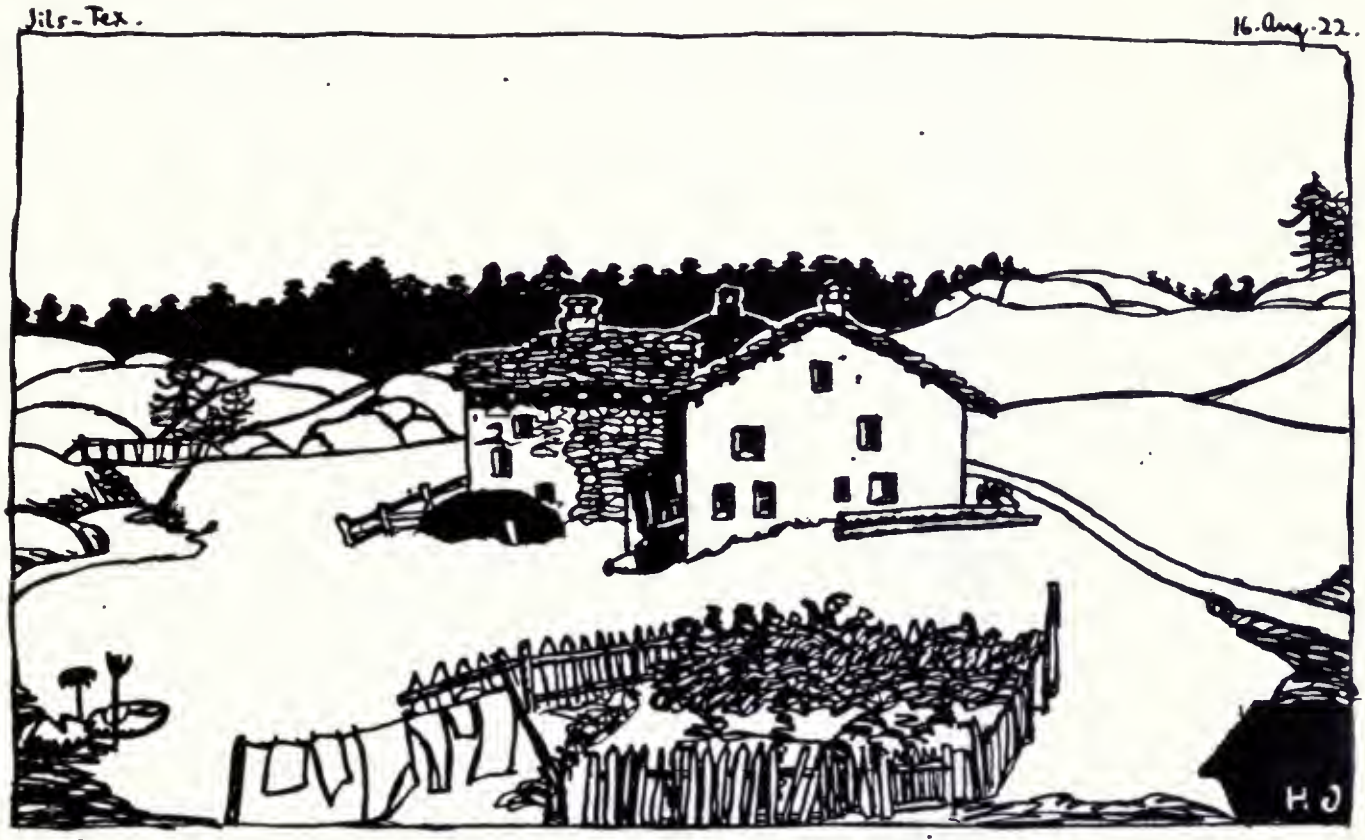
Maher: How long would it take you to sit down and do one of those drawings?

Jenny: One of these? I must show you others. These have to be just Missouri. The ones I took in the tropics and so forth--I don't know--an hour or two.

Maher: What was that like, sitting down and dispatching a picture like that, taking an hour or two like that?

Jenny: Well, that was on a hike, on an outing, on a Sunday afternoon, or so. I have many more. I tore them out. I was also impressed by the fall colors of the eastern coast and Missouri. Here is a tenant farm house near Columbia, Missouri. Here is the old Model-T Ford.

Maher: [laughs] Were you running out of India ink?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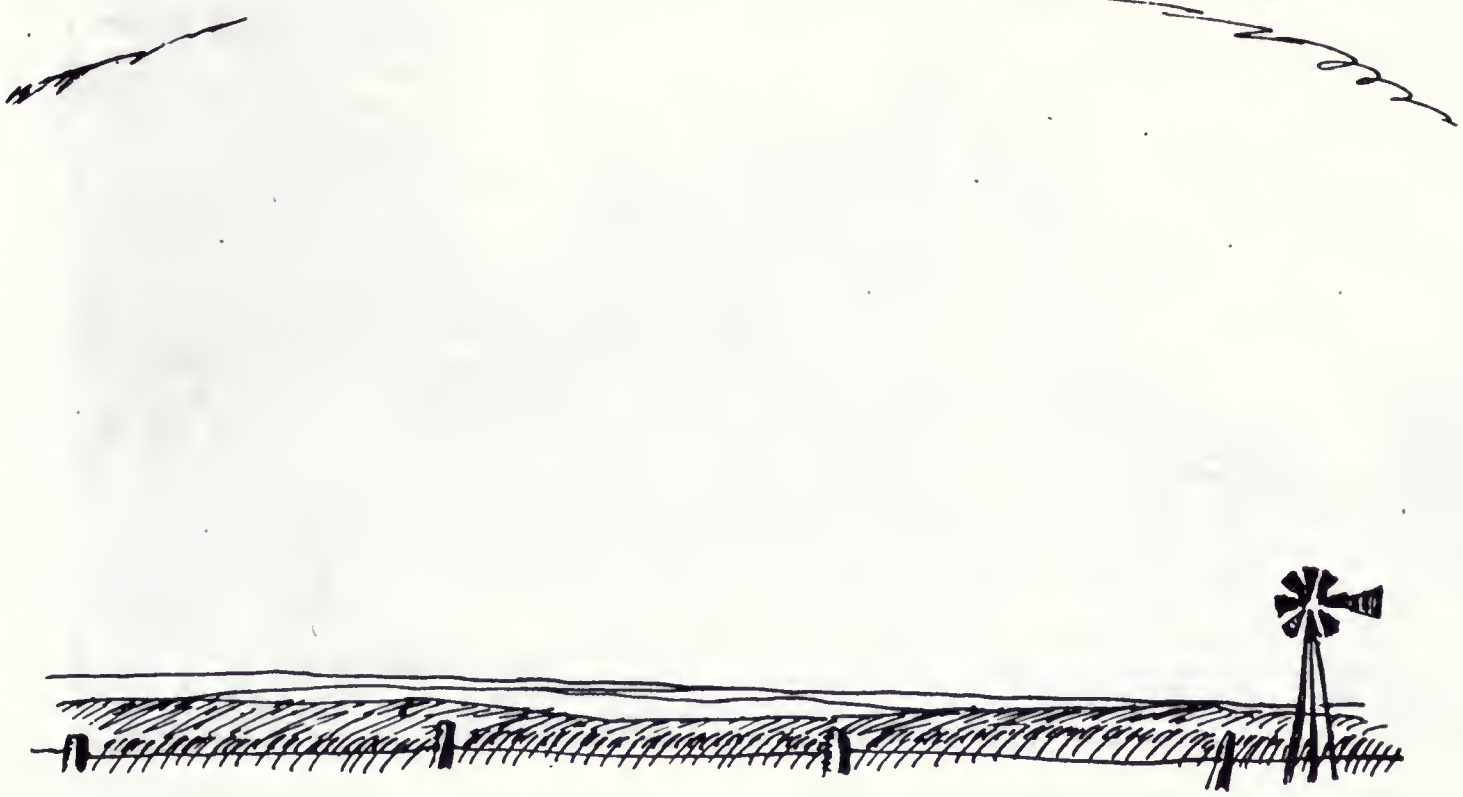
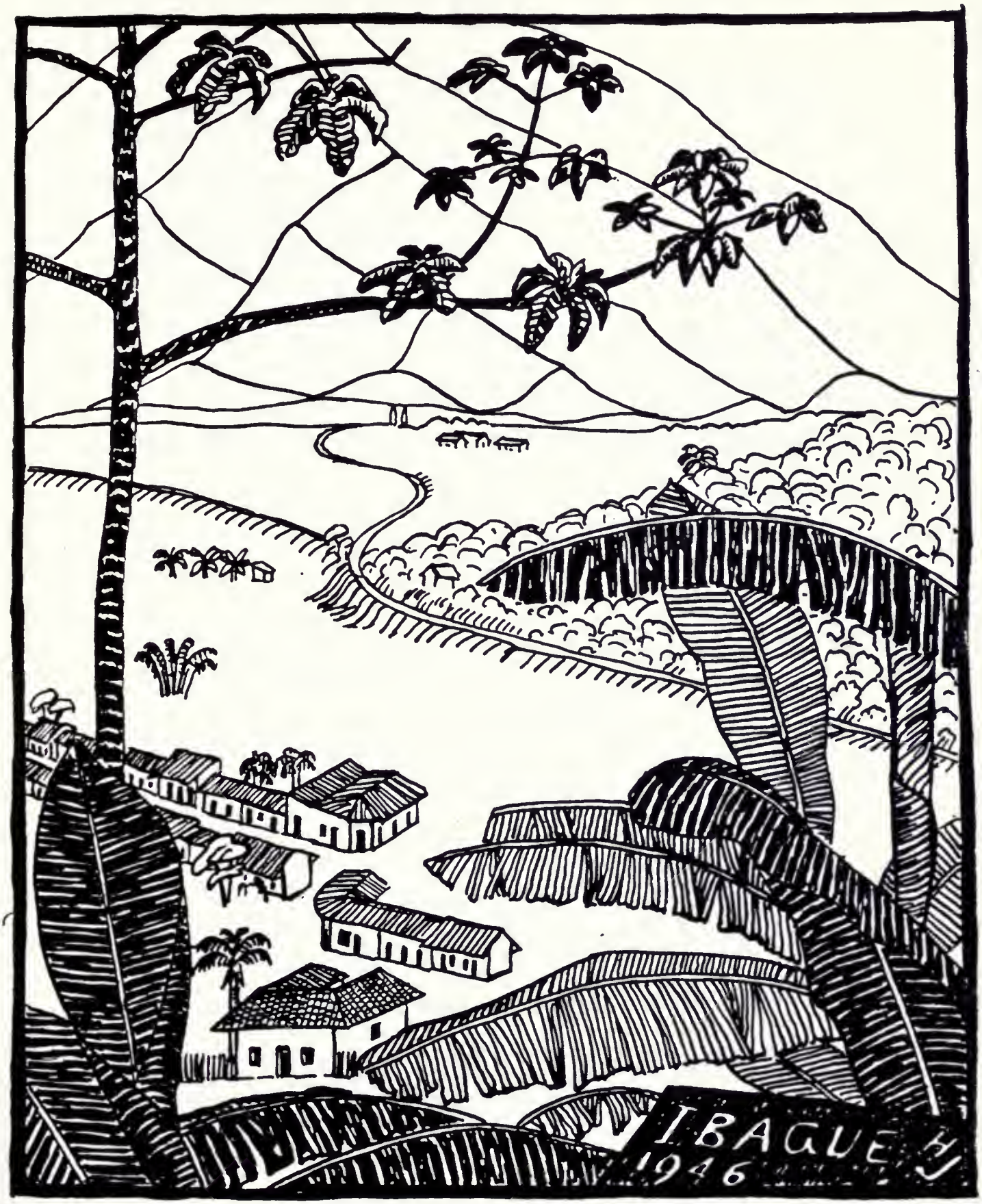


Farmhouse on Knox silt loam, Missouri, 1929.

Tenant farm house
on Lindley silt loam
near Columbia, Mo.

March 30, 1930



Tropical landscape, Ibaqué, Colombia, South America, 1946.

Jenny: No. Here we took a trip to Boulder, Colorado and saw the first cacti. I will show you what I did afterwards. I haven't done anything now for many years.

Maher: When did you stop?

Jenny: Let's see now, I did it when I was in Columbia. I guess I stopped maybe through the influence of my daughter perhaps. My daughter is an artist among other things and when she went to Cal--she, however, was trained to do abstract art. I tried to do that too and I couldn't. She can do both. She does realistic art and abstract art. But I couldn't do it. I was too much conditioned already to what I had been doing and I couldn't do a good job. But I don't know whether that was the reason I slowly quit. Maybe I started to spend more time on writing or was more absorbed in my university work, or what. Or maybe I don't have the relaxation ability it requires. I don't know, but I certainly haven't done it anymore.

Maher: This was partly the reason I was asking earlier about how it felt to be sitting down for two hours--being completely detached from everything. Not so much how was the feeling at the time, but how did it feel to have those periodic respites?

Jenny: I remember the philosopher Schopenhauer. I know him quite well through his writings. He calls it a "state of contemplation," where you just concentrate on something and the world doesn't exist, and that's about the way it is when I make a sketch. I become completely absorbed and the crucial thing is, especially in a landscape sketch, you cannot put down everything you see. So you must start abstracting and, really, the time consuming thing is what elements in that landscape do I want to record, do I want to draw, and what do I leave out, and so it makes it a pleasing thing. Of course, that's relative.

I haven't done that for a long time now. I don't know why. In the country on a nice day if I sit outside, I do not think I want to draw the house. I see a fence to repair. So that's what it might be. So I'm out of it; I haven't done anything. But at that time it was very important.

Maher: Do you miss the effect of those periods of contemplation?

Jenny: No, no. I guess you have that contemplation, too, when you write something, if you have some idea and write it. You concentrate and everything is edited out. Okay, so that's a side glance.

Riverside, California, and Irrigation Experimentation, 1934 and 1935

Maher: Now, maybe we can move on to something entirely different. Maybe you can tell us briefly about the transition that you made to Riverside--the circumstances that led up to your reaching Riverside--and then the work that you did there in respect to the orchards.

Jenny: The year-long stay at Riverside--I wouldn't call it rejuvenating because I was still young [chuckles]--but it had a profound impact, not only because of what I told you about my artistic interests, but also because it brought me face to face with a new world in terms of pedology. The two new things were irrigation practices (the need for water), and the alkali problem. These two don't exist in the humid region in which I had lived for all these years. They were entirely new aspects.

For the irrigation experience--the water problems--I had good contact with Warren Schoonover, who was a university extension soil specialist. I learned a lot from him about the applied phases of the soil water regime; and for the salt problems there was Kelley, mentioned earlier, who was a world authority on alkali soils. I learned many things, which one would never find in textbooks.

I was a person who would write a textbook on soils in the humid region, who might read some papers on alkali and water, without having first-hand acquaintance with it. Here I was confronted with these things; it was something I could see, something I could feel. It became a part of me, and it has been of interest to me ever since.

The appreciation of the theoretical phases of the moisture regime came later through [Geoffrey] Bodman, at Berkeley, who was the professor of soil physics. But the practical questions, the irrigation procedures or the California water plan--what it means and what problems there were--came through Schoonover, and Leon D. Batchelor, the director of the Citrus Experiment Station, who dealt with these problems.

In spite of the revelation of these two great soil problems of the arid region, I did not, in the long run, pursue them--I did not change my direction. Although we started on the moisture problems at Riverside, I did not pursue them later on, because, when I came to Berkeley, there were the soil moisture physicists, and I wasn't appointed as a soil physicist.

Jenny: My appointment at Riverside specified that I would work with Kelley on the one hand, and with Batchelor on the other. The Batchelor part had to do with irrigation. At Riverside the station had a large citrus orchard which was well established in terms of research design, and especially from the viewpoint of statistics, which was a relatively new thing. They had about 200 large parcels which they irrigated and fertilized in different manners, and they had citrus trees growing on all of them.

At the beginning everything was uniform. After a few years, they observed problems with water penetration. In the no-problem soils, where they grew heavy cover crops, and had heavy manure application, they would let the irrigation water run in the furrows for only a day, or half a day, and the water would penetrate to several feet, and fill up the soil's water reservoir; whereas other treatments--say, where they added only urea, or no organic material at all--irrigation required two or three days. They had to let the water run in the furrows that long to get enough water down into the soil to three or four or five feet of depth. Hence, for the practical farmer, there was an enormous disadvantage. The way irrigation districts were operating, the district would give him a flow of water for a given number of hours a month. So with this penetration problem, he could not satisfy all of the water needs of his plants.

The question came up--how was the soil affected? I was working closely with a fellow, E.R. Parker, who was originally involved in the design of that orchard, and we worked closely together. We made exact measurements of irrigation practices: how much water was utilized; how fast does the water run in the furrows; and how deep does it go, measured with augers. Actually the King tube auger was the crucial instrument to get soil samples to a five-foot depth to measure moisture control and other properties. So we did all that while workers were irrigating. Now, with the long King tube made of steel you have to "hammer" it into the soil. And as I was doing that I began to realize that the effort I had to expend varied from plot to plot. It was the least difficult on the manured plots and most difficult on the urea plots and the mineral fertilizer plots. So I devised a scheme to let that hammer drop systematically. I don't know whether you know that a heavy hammer goes with the King tube, and by lifting it to a chosen height and dropping it upon the tube you get a certain amount of energy applied on the tube, and I could get a "profile," a depth sequence of the amount of force needed to push the King tube through an inch of soil. It showed that there were hard layers forming; there was a plow sole forming on the non-cover crop--non-manure plots. So we could establish--by measuring soil densities and all that--we established that there was a deterioration of the soil structure because of the non-use of manure and cover crops.

Maher: Essentially what you had been using was a penetrometer?

Jenny: The penetrometers were later developments.

Maher: Later than that?

Jenny: Well, at least we did not have a penetrometer. I should add that the method with the King tube and dropping that weight on it gave us good depth functions. But it was not the kind of information the soil physicists appreciated because it was not done in terms of their more refined concepts of soil mechanics. Later on, soil physicists repeated our work with more accurate methods without, however, getting additional practical insights. What we did was really an empirical, you might say crude, thing and it worked very well, but it did not make a contribution to theoretical soil physics, to soil friction, and other aspects.

Farmers had been encountering poor water penetration, which was a big thing in the whole of irrigation agriculture. In the early days arid land farmers were always cultivating. Every week they would go through the orchards with discs or plows for water preservation. Before the physics of soil moisture had been advanced, the creed was that you must cultivate. Every week you cultivate the orchard to have a powdery surface soil that would preserve moisture.

Maher: It was to reduce transpiration?

Jenny: To reduce evaporation.

Maher: Only evaporation?

Studying the Effects of Plowing and Cultivation on Orchard Soils

Jenny: Only evaporation from the soil. The greatest water loss was believed to come from evaporation, from soil drying out. But one citrus grower, a Mr. [F.] Hinckley, had stopped all cultivation, and to reduce transpiration losses he cut the grass and mustard that were growing and later he just sprayed them with oil. He had a clean field, which was considered absolutely necessary for tree growth, and he had not used any cultivating tools. He had permanent watering furrows which lasted for ten or more years. His scheme had been going for twenty years, and he had the most beautiful orchard, and he had no water penetration problems. Schoonover had observed Hinckley's way. Schoonover was in on our discussions and in formulating some of the things we did.

Jenny: So we found that manuring and the cover crops prevented the destruction of the soil and the formation of the plow sole layer.

There was another aspect besides the plowing and the discing. Workers came in the orchard with big trucks when the oranges were picked. With big trucks to haul the prunings, and with big sprayers, there was a lot of compaction going on,and we could measure that with our King tube. I could even measure compaction under the hoof of a horse, or how much compaction a tractor caused. We got a clear picture about compaction and water penetration. They were related.

An idea came up. I don't know who had it first--perhaps a practical farmer, like Mr. Hinckley--that maybe we should stop all traffic and cultivation, or have traffic vehicles roll on permanent rails, or something. To prove that point and explore it, we used an adjacent virgin field that belonged to the experiment station. We divided it up and started compaction and discing experiments. In one set, a tractor-pulled disc went back and forth on the dry land for half a day--always the tractor on the same track, and the discs extending outside. We could see that after about two hours we got dust clouds coming up, and after four hours we had to wear gas masks because of the dust. The discing was destroying the soil structure and we had to discontinue working before the day was over. Afterwards, that plot was irrigated and, bang, the water wouldn't go down. You could not irrigate the soil. I don't remember how many days it finally took for water to enter the soil. So there was an experimental way of showing how frequent discing, and presumably plowing, destroyed the soil structure and interfered with water penetration.

We also had plots, a dry plot and a wet plot, where we compacted soil with a tractor. We created that plow sole, and a dense layer about a foot below the soil surface. So that confirmed Hinckley and the other people who thought these operations were harmful. It helped eliminate in California that cherished symbol of agriculture, the plow.

We did an additional thing that took us seven years to complete. We wanted to see whether we could recover that land.

Maher: Which land?

Jenny: The new plots where we had disced the soil and compacted it and destroyed its water penetration. Thus we seeded the plots with mustard, and irrigated long enough to get some water down and let the plants grow wild. We didn't do anything to the crop. With the fall rains the mustard came up again. We made one irrigation during each summer, and penetration was getting better. After seven years we had a good crop of mustard, and the water penetration was satisfactory, about the same as it was at the beginning. We

Jenny: resampled the plots and found that the bad soil features had largely disappeared. So, merely regrowing vegetation, leaving it to decompose, and eliminating mechanical treatments brought about the recovery of the soil.

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Jenny: All I am hearing now or reading is that the plow and the disc are used less and less, and probably there are very few plows left in California.

Maher: In the orchards?

Jenny: No, I believe in general, but I don't know for sure. The experiment served as a basis for the Riverside experimenters and for the extension people to re-think the whole problem of cultivation in an arid region. I should add that at the same time, the investigators at Davis showed that the greatest amount of moisture removed from the soil is due to transpiration by plants, not evaporation from soil. That was an independent type of information that probably helped eliminate the weekly cultivation efforts. So that was another component in this soil management transformation. Now I don't know what the situation is.

Maher: Whose idea had it been to set out and experiment in that virgin area?

Jenny: That was my idea. What they (Batchelor, Schoonover, and Parker) came up with was the initial plan that I should study the experimental orchard plots to determine whether I could detect any soil changes.

Maher: Prior to that experiment?

Jenny: Yes, they had already found the irrigation difficulties, and they wanted to know whether this can be pinned to a soil change. I had help from several people. Fortunately, I did a lot of the work myself. If I hadn't pushed down this King auger myself, it wouldn't have occurred to me to develop this method with the hammer to get a quantitative depth function.

Maher: With respect to the earlier work that you had heard about or that you later decided existed in the development of plow pans, and in the compacting effect of mechanical equipment in the orchards and other agricultural fields I have this question: What was the state of agronomy and soil science in respect to that?

Jenny: At Zurich there was no soil physics (applied soil physics) and at Columbia there wasn't any either. I knew there were plow soles, but that was just a word. I had never encountered them in my sampling of soils. I was not familiar with this thing until I got to

Jenny: Riverside with its relatively sandy, (sandy loams) coarse textured soils, yet without a marked B horizon they had these plow soles. But I learned--I absorbed so to speak--a lot of information about irrigation because I was in the midst of it.

Incidentally, I loved it very much. It was always nice weather. It was hot, but dry, and you could get in the cool shade under a tree and pick an orange any time. It was interesting watching the water flow in the furrows, and measuring with a stop watch how fast it was going. The flow rate was very sensitive; the furrows close to the trees were best, having slow flow, and the furrows in the middle where the traffic was heaviest were the poorest, had fast flow because the water would not enter the soil. It was an ideal set-up. To me it was novel because you got results in the field--not lab data. It was great. I enjoyed it.

Maher: Had you ever heard of anybody doing a penetrometer-like measure?

Jenny: I heard about it later.

The extension engineer in irrigation was highly pleased. He had a new message for the farmers, something which was practical and helpful. So the impact, the follow-up, was really great for irrigation agriculture.

Maher: Did you have enough awareness in the years succeeding that demonstration to notice the movement toward elimination of discing under the orchards of California?

Jenny: I did not observe it, but I heard of it. Many people told me about it.

Maher: That was under orchards?

Jenny: Yes, it must be under orchards. It must be with a permanent crop, yes. I don't know how it is now with cotton. They still have to prepare the land. I don't know how it is done, but I'm sure they must be aware that they should do the least amount of disturbing the soil.

Maher: The reason I am asking is that a large realm of recent research that I am sure you are aware of is directed toward no tillage or minimum tillage of crop land. That's getting to be a big deal. They used herbicides other than oil to kill off the vegetation and so forth. But what is interesting to me is that recently in a soil science encyclopedia, I read a summary view of zero tillage and its background. As to this history of that idea of challenging the use of the plow, they say it just goes back to the sixties or seventies when Rothamsted studies were begun.

Jenny: In 1960 or 1860?

Maher: No, 1960. It's ludicrous the way they are speaking about it. It's almost like they are gathering up all of the history as to the background of zero tillage and the farthest that they can go is a mere twenty years or so ago. But the reason I am questioning you about the orchard is that essentially you had been researching that very discovery with respect to orchards. Evidently, the lesson was learned throughout the orchard industry in California--a very important industry. So I am curious whether there was any connection or any spill-over into crop land practices.

Jenny: At that time?

Maher: Anywhere along the line.

Jenny: I'll tell you, this is really an important point and Schoonover is still alive. He lives in Santa Rosa. I haven't seen him for many years. It would be interesting to hear his story, whether he followed that up. I don't know what the spill-over was from the orchard work to the field crops. For years the Soil Conservation Service sent reprints of our paper to their field offices.

Maher: I have one more specific question. When you came up with seeing whether the land could recover--whether those plots could recover from the damage that was done--whose idea was it to broadcast some mustard and leave it?

Jenny: I worked with a man named Parker whom I liked very much and he liked me. We discussed this project and the operations all the time. Now, I know specifically that I came up with the idea of running an experiment. Whether the crop-planting follow-up was wholly mine or wholly his or we came up with it jointly, I don't recall. It got to a point where we were just one person actually, always exchanging ideas. The one thing that impressed me greatly with the whole activity was that I just had to whistle and I got help from field men, or Dr. Batchelor provided equipment and tools and all that. I never have experienced that before or after, to get all of this instant support. I could see later, when I learned more about California, that the entire set-up was really a very crucial study from a practical point of view. I didn't know that when I was involved in it.

It's still a big story. I know one of the soil extension men at Davis, [William] Wildman. He goes around and lectures about compaction and all that. Others repeated the soil studies in other parts of the state.

Maher: The person at Davis that you mentioned, had he known of your work in Riverside?

Jenny: I didn't ask him. I doubt it because he came to California only a few years ago. He got his degree at Davis. No, he probably never heard of it. Anyhow, that's farther back than the customary five years. I don't expect my old stuff to be read.

Maher: So you don't really know the effect of the work done--just waiting to see whether it could recover?

Jenny: I don't know, I don't know. I should say that my friend Parker had no soil knowledge or thinking. He was primarily a horticulturist and a statistician. He had a lot of know-how with field experimentation that was just ideal. We never published our paper until the seven-year study was finished. I put his name first; he remonstrated, but he was short on publications.

Maher: Did you ever publish the results of that crude penetrometer or the idea of trying to measure--

Jenny: Oh, yes, yes, that's in that same Soil Science paper. It's all in there.

Maher: Is there anything else from your experience in southern California that you would like to point out or expand on?

Jenny: I did not do any alkali work, but, of course, I went on a lot of field trips to areas with salt problems. They showed me a lot of things. As I said, the experiment lasted seven years, which means that I was at Berkeley most of the time. Every year I would go south to Riverside once or twice, which was always nice.

The work with Kelley had to do with the ion exchange and moisture adsorption on powdered crystals. That's another topic.

IX THE UNIVERSITY OF CALIFORNIA AT BERKELEY, 1936-1967

[Interview 8: June 4, 1980]##

Professor Dennis Hoagland's Influence

Maher: You came out to California once again and this time permanently. Could you tell me a little bit about the circumstances whereby you came to the University of California at Berkeley?

Jenny: As I mentioned once before, when Dean Mann came to Zurich to invite me to come to the United States, my first choice was to go to work with Hoagland in Berkeley. But that was not feasible, so I went to Waksman in New Jersey. Then a year later, Hoagland was on that international field trip we discussed. He kept very much to himself on the trip and people called him a cold fish. When Kelley wanted me to come to California permanently, Hoagland's department was the logical place, because I did not want to go to Riverside. I would not have moved from Missouri to Riverside, because at Riverside I was told by Kelley that they were against theoretical research. They were emphasizing practical research. In fact, several years later there was so much pressure against Kelley, maneuvered by Schoonover, I was told, that he was transferred to Berkeley and he became head of the new, small soil chemistry department. So Berkeley was the outlet. Hoagland's group was the outlet.

When I joined Hoagland, it turned out that he was anything but a cold fish. He was reserved, but he had very strong personal interests in his associates. He had strong feelings for a person, but also against a person. There is the notorious case of Dr. W.F. Gericke, who "invented" hydroponics, the large-scale growing of plants in solution. He and Hoagland were at cross purposes and Hoagland had him fired. Gericke was a full staff member, yet he lost his job. I always felt it was too severe a punishment.

Maher: You say that you always felt that that was too severe. Does that mean that those events occurred when you were at U.C. Berkeley?

Jenny: That was in progress when I came here. It was in full bloom, so to speak. Hydroponics became important, and Gericke somehow kept secret, or did not reveal, his combination of ions which he added to solutions. Hoagland objected to that and went through the dean and Gericke was fired. Some years later I was on a committee to consider having him back, but the committee turned him down again. I felt it was a tragic thing because the man, advanced in years, was ruined financially.

Maher: Was it intimated that he had had some ambitions to patent this privately or something like that?

Jenny: I don't know. There was some rumor that he had ambitions for sort of a kingdom, but that's all hearsay. He was sort of an outspoken person and he and Hoagland didn't make it. So Hoagland became sharp and worked for his dismissal. Incidentally, Gericke was a member of the experiment station's staff, not of the academic staff, I think. So there was no problem with the faculty tenure. Otherwise, dismissal wouldn't have happened so easily. Later on Hoagland and Daniel J. Arnon re-investigated this whole hydroponics scheme to check some of the claims and to give a more thorough, technical, scientific background to it. They wrote a bulletin about it and it was a popular thing.

Gericke also had his hands in industrial developments. He was a consultant to a group in New Jersey who had large-scale hydroponics. And whenever it rained in New Jersey, things went haywire, and he was called to fly to New Jersey and straighten out the pH changes, or something, because it was outdoor hydroponics. So when I came to Berkeley he was still around in the department, but pretty soon he was eliminated.

Hoagland was on many important committees. He had "only"--in quotation marks--a master's degree. He didn't have a Ph.D. He brought that up ever so often, so it must have bothered him, but it certainly did not affect any of his scientific qualities. At that time, he and [William H.] Chandler had just discovered that zinc was an essential element in peach orchards. That was a big thing in minor element or micro-nutrient research. Also Hoagland had made a name with the idea that ion uptake by plant roots is a metabolic process. But Roy Overstreet and I felt that he was scientifically not broad enough; we felt that way because of a fellow in Sweden, H. Lundegårdh, who had creative ideas on ion uptake by plants. Hoagland would not accept them and would not even discuss them. Roy and I felt that, in the long run, it was a very productive group they had in Sweden doing that kind of work.

By that time, Hoagland's daily schedule was full of committee work; he was on the important budget committee, and other things, and he was helpful in financing a greenhouse for [Albert] Ulrich and myself. He had a knack of getting very good people. He had an

Jenny: outstanding staff. It was really an intellectual group, very stimulating. We knew each other--what was going on, what this guy was doing, about the other guy, and how it was coming along.

Maher: Why did you know so well?

Jenny: We talked to each other. We were all on the same floor, and it must have been helped by the spirit of one or two people. One of them was [William Z.] Zev Hassid, who came around to talk. So that's what it really was. There was Perry Stout and, of course, Roy Overstreet. There was this continuous exchange of ideas and stimulation about what was going on. We were happy when something was discovered or found.

The first thing every morning Hoagland would go to the greenhouse and spend an hour there discussing everything about the plant experiments, and he invited us to come along. We were always involved in all of the arguments. So it was a fine thing.

Maher: In retrospect, what do you think had caused people on that transcontinental journey to think that he was a cold fish?

Jenny: Because he was reserved and didn't say much. Of course, the people of the congress looked at soils and he was not a soils man. He was a plant nutrition man and basic nutrition topics were not discussed. So I think he must have been bored a good portion of the time.

Maher: Why was he there in the first place?

Jenny: He had an interest in soil fertility. He was interested in potassium fixation in soils, but <u>not</u> in pedology, the profile descriptions and all that. He was at the congress in Washington and there were many topics in his line, so that part was all right.

Maher: As a plant nutritionist he was perhaps unique and important in the sense that he viewed plant nutrition as something that occurred <u>in soil</u> perhaps more than others. Is that correct?

Jenny: He had an interest in plant nutrition in soils although all of his work was in solution cultures because he felt that's more easily controlled. But he was concerned with soil fertility.

The Conflict Between Plant Nutritionists and Soil Technologists

Jenny: When I came here there was a considerable conflict between the plant nutrition people and the people in soil technology. Charles Shaw was the head there and Earl Storie and Walter Weir and Jeff Bodman

Jenny: were in that group. There was considerable animosity, and I was told, it originated as follows: Shaw was in a way a successor to Hilgard's work, but he did everything in opposition to what Hilgard believed. He was an outgrowth of the new soil survey group that originated in Washington, D.C. in 1900 and he was mapping California's soils. After the First World War there was a move to settle veterans on agricultural lands in California. A committee was appointed, and Shaw was put in charge of selecting an area in the state suitable for settlement by veterans. The area he selected as having first class land was near Delhi, California. In this committee he excluded the plant nutrition people, which they resented.

The veterans planted peach orchards and then it turned out, after a few years, that it was not successful. The trees died. Then Hoagland and Chandler got interested and they found out that the trees suffered from zinc deficiency. Well, this showed that Shaw and the soil surveyors didn't know enough about what is good for plants. So there was this friction; it was very strong. But what was new to the plant nutrition group was that I also had these pedology interests. So I went back and forth with Shaw, and in my seminars I brought these two fields together. Hoagland had to realize that Shaw's group existed and had something to offer, and Shaw realized that plant nutrition, of course, had something to offer, too. Shaw once said that I was going to be a new Dukuchaev. So he was favorably inclined towards me as was Hoagland. This was a good constellation to bring the departments together.

Later on, when Shaw suddenly died, Overstreet and I moved to the new department of soils and Kelley was put in charge. But more about that later.

The Soils and Plant Nutrition Department

Maher: I'm trying to understand the sequence of Hoagland's appreciation of soil. From some of the things that you have written and my impression of plant nutrition, in general, is that the tendency is, because of the experimentation being much better, to focus on experimentation through a nutrient solution and that leads to difficulty when you are dealing with actual soils. Yet Hoagland at least was willing to recognize and reach out towards soil. Can you mark the growth of Hoagland's initial understanding and appreciation of soil?

Jenny: He did not have the idea of soil as--in quotations--"a natural body with horizons." To him, soil was what we may call "soil material" which you would handle with a shovel or a plow, and that's what it was. But he was aware of a lot of soil problems. He knew, for

Jenny: example, that on the red soils of California you could not grow truck crops without phosphate fertilization, but tree crops would grow without it. He knew the problem of potassium fixation. Professor John Burd, in his department, had worked a long time on soil solutions, extracting soil solutions and analyzing them to see what was in them.

Maher: When was that?

Jenny: That was in the 1920s. This was important work. So when I came, his work was in soil solutions and because Burd worked on water extracts of soil, it was natural for Hoagland to use those in his plant experiments and to make artificial ones. So when I came with my ideas of using clay suspensions this was in a way challenging other methods, and he was the man who set up most of the objections to the contact theory, but in a positive, constructive way. He said, "You've got to show this and this," and they were all very valid points. So this was a very good thing.

Hoagland had a broad view. At the same time I was there, he hired [Horace] Barker as a microbiologist; Hassid was a biochemist. Dore was an x-ray man in cellulose structures. So it was a diverse group.

Maher: So you are saying now that he was broad in one respect. Earlier you had said that you and Overstreet had felt that he was narrow. Could you distinguish between them?

Jenny: Yes, we felt he was narrow in theoretical concepts. He would not talk about electrons and respiratory chains, and the biochemical wording was not up his sleeve. That came too late in his life. But we young people leaped on that and Lundegårdh leaped on that. So we felt he was not stimulating theoretically.

Maher: He was broad in respect to--

Jenny: In topics but not in the theoretical impact of these topics.

Maher: Was it through the avenue of personal relations that he was able to identify a broadly diverse people to come to the department?

Jenny: I think that came out from his interest in work. Plant nutrition was really the old agricultural chemistry under Hilgard and they had had a fertilizer department, they had had a soil chemistry department, and under Hoagland that was all combined. I had forgotten his background--his master's degree and where he got it. I think he got it in Colorado some place. But he had, you might almost say, an agronomic background and at Cal he deepened it with fundamental experiments. But he was strictly an experimentalist. He would not come out with theories. He was not favorable to theories.

Teaching Colloid Chemistry

Maher: Let's go back now to the mechanics of your coming. What was some of the mechanical decision making involved in your acceptance at the university?

Jenny: First a position had to be open, and at that time, Mr. [Paul] Hibbard was an experiment station chemist who did a lot of analytical work. He is the one who found, for example, that zinc might be the element that is missing in the Delhi orchards. He retired and that position became open. So there was an opening. However, this position was in the experiment station and I had to have a position on the faculty. So that position had to be converted to an academic position and the key--the lever--was to get something in colloid chemistry. That was needed and all of the agriculturalists and many others agreed that would be the thing to have. There was need for such a course and to develop such a field.

Maher: So they were willing to give up an experimental position, to have it go over into the academic.

Jenny: That's right. There was a switch.

Maher: Was that unique?

Jenny: No, I think that situation happens all the time, but there is a lot of opposition to it from the whole academic university body. We have the case now with Rod Arkley. He never made it. Barshad never made it. There are quite a few cases where these experiment station men became scientifically qualified in terms of creativity and outlook, but couldn't make it because agriculture would have too many faculty positions. Especially the people--the literati--wouldn't want that. When I first came there was a saying in the Faculty Club that when a new face shows up, it must be in agriculture. Later that changed. A new face was in radiation. During the 1930s other departments declined; Latin and Greek declined each year, and agriculture prospered, so there was this antagonism.

Maher: So the focus then moved to the teaching of colloid chemistry?

Jenny: That's right.

Maher: Maybe we can discuss that. Still keeping the focus narrow, what were the immediate intentions of the new proposed course? Who wanted this course to be taught, who had control over what its content would be, and for what reasons were they being proposed at that particular time?

Jenny: Burd, in Hoagland's department, was working on phosphate problems. He worked with the Freundlich isotherm, and this all had to do with colloid chemical reactions. Then there was an important person in home economics, Fay Morgan, who was a professor of nutrition, a chemist. She realized that the students needed colloid chemistry. All of their problems are not answered by biochemistry of the 1930s, especially the problems associated with conserving food, or making wine or whatever, the colloid aspects were needed. There was also the Department of Food Products. They needed something about gels and suspensions. Wine isn't a pure solution. Most of the products are colloidal systems, as are clays and humus.

Then Hoagland had a close friend in biochemistry, C.L.A. Schmidt. I met Schmidt earlier when he came to Missouri once and he was interested. Schmidt had an associate, Professor [David] Greenberg, who worked on Donnan potentials and swelling of proteins. And before Schmidt, there had been the famous [Jacques] Loeb, who worked on proteins and Donnan membranes. So there was a long tradition to provide support for colloid chemical work and somewhere a center to house this and make an issue of it. So this was a good constellation. Everybody wanted it.

Maher: So up to this time, there was simply no place on the campus where you could go to get the word on colloid chemistry?

Jenny: That's not quite so. There had been activity in the chemistry department under [Joel] Hildebrand. I believe Hildebrand had worked at one time on the effects of salt and he had certain ideas. He was appreciative of colloid chemistry. But I was told he opposed this move to have colloid chemistry in plant nutrition. He probably would have been in favor of having it in chemistry, but the constellation in chemistry was not favorable for that. The emphasis in chemistry was on other things. I don't know whether he had something against me personally or something from my background. But he was overruled and so the position was created.

Maher: "From your background"--what might that have meant? You say that you did not know whether he had some objection to you personally or from your background.

Jenny: That was just hearsay, what people like Hassid mentioned. My background was not in thermodynamics. He could have believed there was not enough theoretical physical chemistry in what I did. The thing that impressed people was that earlier I had given a seminar on my ion exchange equation plus its relationship to flocculation and dispersion and the stability of colloidal systems. That integration made a very good impression because that was exactly what they wanted. But for Hildebrand perhaps it wasn't theoretical enough. I didn't discuss quantum theory. I had no background in quantum theory. But this is all hearsay. I don't know.

Maher: Did he attend the initial seminar?

Jenny: That I don't remember because I didn't know him. I don't know whether he was there, but afterwards he became friendly, because I immediately used the radioactive techniques that came out, and I got a good reputation. Faculty members had a ski hut and I went there and went skiing with him and we washed dishes together and discussed things. So I think it was all right.

Maher: I have a kind of parenthetical question. When the applicants were being interviewed, they would come and present a seminar. Was that the set-up?

Jenny: That was in that case. I gave two seminars: one on that ion exchange thing and one on soil chemistry in relation to pedology, especially to soil formation.

Maher: These seminars would be attended by what sorts of people? Would they be from many departments?

Jenny: It was done like we're used to all the time. It was announced in the department and invitations were sent to many other departments. But I think it was more by word of mouth.

Maher: So the demand by a good number of people from various disciplines was definitely there.

The Status of Colloid Chemistry in the United States

Maher: What was the position of colloid chemistry at this point as a science, as a study?

Jenny: Of course, it had always been very strong in Europe and a center in the United States was at Cornell under the leadership of Wilder Bancroft. But somehow that wasn't really an outstanding success. Bancroft, I don't know the details, [pause] I shouldn't really mention this without thinking more about it. But a colloid chemistry conflict developed between the United States and Europe and it centered on a famous European Nobel prize-winning chemist [Richard] Willstätter, and on a group at Cornell. The way I understood it, Bancroft was for Willstätter, who believed everything was adsorption, whereas the Cornell group felt they had crystallized certain enzymes, which Willstatter disputed. But it turned out later on that the Cornell people were right--but not Bancroft. So this was an open conflict that hurt colloid chemistry in the United States.

Jenny: A strong center existed at Wisconsin under [R.A.] Gortner. He worked on organic colloid chemistry and he had certain ideas about proteins. That was also in agriculture. Then here at Berkeley Hildebrand did some and also [William C.] Bray. Bray became a real friend of our group. Bray was a professor of chemistry who, during the war, had worked on iron oxide and got nowhere with it. He appreciated the complications. He made iron oxides for the catalysis of something and he had a hard time standardizing anything. Now we know, of course, he started out with amorphous systems; they slowly crystallized; and had all of these different modifications and he could not pin them down. In those days, x-ray analysis of colloid particles was not known. So he was appreciative of what we were faced with. He was always really constructive and helpful.

Maher: In your discussion of Europe versus Cornell, there seemed to be a dispute over whether there was a colloidal-sized particle being produced of crystalline quality.

Jenny: That's right.

Maher: Was that the issue, the matter of crystallinity?

Jenny: That's right, that was the issue. Willstatter thought the enzymes were an adsorption phenomenon and he tried to separate them by adsorption techniques with aluminum hydroxides or aluminum oxides. The Cornell group was able to crystallize it.

Maher: So he felt that particles with some enzyme aspects to them of colloidal size must have been a previously colloidal sized particle with molecules of the enzyme adsorbed to them. Is that correct?

Jenny: I can't answer your question without really reviewing for myself what the situation was. That was in the twenties. I was still abroad and when I came I didn't go into that. That's just what I heard about it. So I should not go into that. I'm sure that has been discussed somewhere, but it is an important thing. There was a later parallelism with genes, that genes were not really molecules or bodies; they were constellations of organic molecules. At Berkeley we had this gene conflict. [Richard B.] Goldschmidt was here and had these ideas. So it was full of arguments and full of pros and cons and it depends on which side you are, and so forth.

Course Content, the Students, and Personal Growth

Maher: So let's get a little bit closer then to your actual teaching of that course and the choice of subject matter as to what it would include and what its reaches would be.

Jenny: Essentially, what I discussed was what I knew myself. But I did familiarize myself with topics which we had not studied at Missouri, as the Donnan system and some of the thermodynamic derivations of adsorption that went back to [Willard] Gibbs. I was always reading the latest literature on all of these things. I discussed the proteins which we didn't discuss in Zurich to any extent, and the cellulose models. So it was a very up-to-date presentation and students appreciated it and were eager to come and learn.

A few weeks ago I was amazed. We were in the space laboratory and discussed something about optics. A fellow said, "I took your colloid course and I remember what you said." He remembered how I had explained diffraction phenomena. So this course had an impact. Then I had a strong influx of people from sanitary engineering. They were concerned with colloidal systems--sedimentation in tanks. They were very active participants.

Maher: During this period it seems like colloidal chemistry was making huge growth internationally or in the scientific world. There was evidently, from your writing, a colloidal chemistry course taught at Stanford; perhaps it was quite important to the progress of the scientific community at U.C. Berkeley at large that a colloidal chemistry course was actually being taught at that time.

Jenny: That's right. James McBain was at Stanford. He had a department and several associate professors, and Horace Barker, who also came in 1936, had gotten his Ph.D. in colloid chemistry at Stanford. But he was hired by Hoagland as a microbiologist. Yes, McBain was an important figure there. He worked on soaps. When I wrote the syllabus, I sent him a copy and he wrote me a very nice letter. He fully approved the topics I covered.

Maher: Was that what led to the publication by Stanford of your--

Jenny: No, I showed it to him after it had been published. No, that came about because Stanford Press sent a fellow around who told me that they had a neat technique of making syllabuses.

Maher: Was the U.C. Press not in existence at that time?

Jenny: The U.S. Press was in existence, but I think the U.C. Press has always emphasized the humanities.

Maher: I was asking about a couple of things in that question. One of the other things I was asking about, wasn't it somewhat critical to the progress of the scientific community at U.C. Berkeley that a course be taught in this area at this juncture in the mid-thirties?

Jenny: I would say the climate was right for it because the people needed it. However, I think, for the future of colloid chemistry it would have been better if the course had been in the chemistry department.

Maher: Were there many chemistry students who came down?

Jenny: A lot of chemistry students came down to Hilgard Hall, yes. Years later, in Hawaii, a fellow who had taken my course and had a degree in chemistry said, "Your course was the only course that was ever helpful to me, because classical thermodynamics, while it's very important, is difficult to apply on practical problems and, everybody has to deal with flocculation and suspension and stabilities the moment you go into the industry." So that's why they came.

Maher: Although it wasn't a lab course, you had a good number of demonstrations.

Jenny: Yes, during every class hour I spent time on demonstrating the fundamentals so the students would know exactly where the ideas came from. For example, the idea of the electric double layer. I spent some time on its origin rather than going into the mathematical theory of the double layer which perhaps in chemistry they would have stressed more than I did. But my course was not really for theoretical chemistry students, but for these other fields.

Maher: In the theoretical chemistry courses, were they divorced from a laboratory base to their discussions?

Jenny: In terms of colloids?

Maher: Yes.

Jenny: Bray worked during the war and he didn't touch that anymore.

Maher: The First World War?

Jenny: Yes, and Hildebrand--this was just sort of an excursion of his. He did not really open the field or command a field in terms of reputation. This was sort of a side trip of his. So the students wouldn't hear anything about it.

Maher: Maybe it was the demonstration, the concreteness of it, that made it pedagogically sound and appealing.

Jenny: That may be so. People like Arkley, who took the course, still say it was very hard work, because I presented a lot of material, and I always handed out problem sets--and not easy problems at that, but they were good problems. Overstreet helped me a lot with the problems. So it was a demanding thing, and I think that satisfied the chemistry department. I'm sure the chemistry students told Hildebrand what I demanded.

Maher: How long did it take you to develop these problem sets?

Jenny: If I look at the syllabus--I had a good number of problems in the syllabus. I must have developed these problems in the first two years. Later on, I went more into optics and I had a beautiful set of demonstrations on optical phenomena with colloidal systems, showing in class streaming double refraction and basic optical concepts. It was a good show. [pause] The fact that it was successful showed up in that, although I was hired as an associate professor at the beginning rank, in no time I was made a full professor. This was unusual. At the time I became full professor, only two people were promoted to full professorship. The other guy was in chemistry, [Gerhard] Rollefson. Everybody who did not know what I was doing was surprised. Besides, Hoagland was on the budget committee which certainly must have helped.

Maher: You say that you developed these problems with Overstreet. Was he right there from the beginning of your being there?

Jenny: He was right away helping me out and he had his own interests; I set up the problem and I gave them to him to solve. He would improve on it, bring out where it was two-faced or not clear enough, and so forth. So he was a great help.

Maher: So these problem sets were very much a matter of growth for yourselves as well.

Jenny: Oh, yes. Yes, I studied a lot for this work.

There were students from entomology. Their department had a section on toxicology, so I discussed surface tension and angle of contact and related things. It was just great for them.

Maher: What were some of the strides that you feel you made in your understanding of the subject matter by developing these problem sets?

Jenny: By developing the problems, I became more deeply concerned about understanding. A lot of the work on surface tension has to do with oils, and a lot of work was developed by a school in Chicago, and there was a lot of literature. Langmuir worked on surface films. This was all new to me, and to absorb it I worked on good questions.

Maher: Parenthetically, was that the Langmuir who was working in private industry with G.E.?

Jenny: Yes, that's right. He got a Nobel prize for his work--the Langmuir balance for study of surface films.

Maher: I asked you what were some of the areas in which you were expanding your own understanding by virtue of trying to develop these problem sets?

Jenny: Okay, one was the whole field of surface chemistry. Another field was colloid optics, and I really had to do that by developing the experiments myself to see whether I could demonstrate these phenomena. I would say those are two areas about which I had very little experience. Another one was the Donnan systems; in fact, I had been given negative impressions about it in Zurich.

The start in Berkeley was a great period. I'm surprised, looking back now, how I did all that and at the same time had all of this pedology work going. It's amazing. I must have been floating in air or something! Now everything is slow because I am much more critical than I was then.

Moving Toward More Detailed Research

Jenny: Much of what I did, especially in research, was what I would call impressionistic, just with big strokes an outline of something. That wouldn't be enough for me now. It would be pinned down into the hard core.

Maher: What in your life do you think caused you to shift from the broad strokes to a feeling that it was necessary to pin it down to hard demonstration?

Jenny: I think that became a defensive attitude. I was being attacked and questioned and criticized. This forced me to be minutely concerned. If I wrote something and was not sharp enough in the precise words they would pick that up and make a big thing of it. So I've become much more cautious and careful, checking and double checking. I got confirmation later when I worked together with a mathematician, B. Schneider, in Germany. I have always been impressed how mathematicians can come up with clear solutions that are absolutely tight. I saw how he did it. He would check <u>everything</u>. He was checking backwards and forwards, to be sure. He didn't shake it out of his sleeve. So that confirmed what I did and emphasized this approach.

Maher: When was this relationship?

Jenny: Oh, with the mathematician? That was when I was in Giessen, Germany in 1963, so that's pretty late.

Maher: Without getting into the details of the particular subject matter that you published and found criticized, can you generally point out a material where you were sobering up about this backlash from publication and getting more critical about attending to detail and getting it as tight as could be?

Jenny: I would say in the earlier Berkeley period. In Berkeley I started with the meaning of the state factors. That had to be explored. Then also with my dealings with Overstreet. Overstreet was very helpful. He would simply challenge many of the things I said. That made me be more careful and more precise.

Maher: So this would be the last four years of the thirties?

Jenny: Yes, I came in 1936.

The Shift in Colloid Chemistry Courses

Maher: What would you say was the peak of the interest and enrollment in your colloid chemistry course?

Jenny: The peak? [pause] I'm not very good at dates. I would say before the war, just before the war.

Maher: What do you think were the reasons for the decline?

Jenny: One very clear reason, the chemistry department hired a fellow to teach surface chemistry. His name is George Jura. He is still on the faculty.

Maher: When was that?

Jenny: I guess it must have been during the war or shortly afterwards. The war upset everything because the interests shifted. The activity shifted. It was a very upsetting period. I know they started working on viruses, and the biochemists started working with Stokes' law and sedimentation velocities, and all that had a big upsurge. So they established their own colloid-oriented courses. The home economics students had the greatest difficulty following my course because their background was perhaps not as good as that of the others. So they started to teach their own colloid topics under a different label or include it in their regular courses; I know material was lifted out of the course syllabus. Bodman, in soil physics, and Ken Babcock, in soil chemistry, presented and taught interesting and prominent topics. But the decline was not disastrous, say, from eighty it went down perhaps to fifty or so; still a good group.

Maher: Did the adoption of material in other courses begin prior to the war?

Jenny: I would say, yes. But I would have to look through all of the class record books and I am afraid that after I retired a lot of stuff had to be thrown away.

Maher: I am trying to distinguish between a kind of a natural intellectual and academic growth which would have encompassed the chemistry department bringing in a colloid chemist and others.

Jenny: Material science also brought in a colloid chemist, [D.W.] Fuerstenau. I think my course served its mission of spreading ideas and making them respectable. I think you were right in pointing out the value of the demonstrations I had. I always had young faculty members around, even older ones, who attended these lectures. These presentations showed what the important problems were.

Maher: Isn't it unusual for faculty members to attend the lectures of other faculty?

Jenny: I can't answer that. I had the same thing in the pedology course. Staff people would attend, geologists, like [Clyde E.] Warhaftig, or botanists.

Maher: What were some of the formal courses that you actually attended from beginning to end in these first years?

Jenny: When I developed the lecture on colloid optics, I took the relevant courses in geology and two semester courses in optics to understand mineral optics. I attended a field course in geography under Carl Sauer. I attended mathematics courses. I could do more of that at Missouri because then I was at a lower academic level. Here I soon became a full professor and then it wasn't so easy to go and audit a course in some other department.

Maher: Why?

Jenny: Because of your status, as a status symbol. You were supposed to know everything.

Maher: Was that a difficulty for <u>you</u> or was that a difficulty for other people?

Jenny: No, it was a difficulty for me.

Maher: Did you feel caught in the bind of being supposed to be knowing more than you actually did?

Jenny: There is always the question of having enough background. When you take on the course you don't have the same background as the students and it may require an enormous amount of work to keep up. The optics course required hard work just to keep up with the students, without the requirements to take the exams. So I don't know.

Work with the Oil Industry on Ammonia Gas Fertilization##

Maher: Can you explain the timing and the mechanism whereby Shell Chemical Company in San Francisco brought a project to the university that you became involved in?

Jenny: The way I understood it, the oil companies had a period of low profits with oils and they were looking for other avenues of income. One was the fixation of air nitrogen because they had the hydrogen gas and thus could produce ammonia. It occurred to the Shell people that instead of converting ammonia to ammonium sulfate or ammonium nitrate fertilizers, the ammonia could be added directly to the irrigation water, and that irrigation water would flow to all of the fields, which should be more suitable than spraying fertilizer all over and getting pockets of high salt concentration.

The company had a very good chemist who was developing these things--I think [Ludwig] Rosenstein was his name. He was aware that the solution of ammonia is slightly alkaline and that alkalinity might have an effect on soils and plants, and so he came to Hoagland, an authority on plant nutrition, about studying these things. In 1936, as I arrived, the company set up a fellowship. It was turned over to me and I started working on it, continuing for about four years, first with A.D. Ayers and then with J.S. Hosking, a student from Australia. We did a large number of experiments with many types of soils. Of course, in those days we still thought that an acid clay is a hydrogen clay. So, we could show that the more acid the soil, the more readily the ammonia is taken up; once held by clay it wouldn't diffuse, it wouldn't go very far. When that reaction picture became clear, the colloidal particle with hydrogen ions reacting with ammonia in water, there is this reaction: $NH_3 + H_2O = NH_4OH$; NH_4OH + H-clay = NH_4-clay + H_2O. Then it occurred to me (I don't remember how) that maybe this process could be done in a dry state, that a dry clay would have hydrogen ions, and NH_3 gas molecules could hook up with the hydrogen ions and make NH_4^+ ions, that is, become exchangeable ammonium ions.

Maher: You would still have the same charge?

Jenny: You would still have the same charge, but it would be a different reaction than the one in solution. So that's what we tried. I think it must have been with Hosking, so that was after Ayers had left. It would have been 1938 or so. So we ran the experiment and we could show it worked. The soil contained exchangeable NH_4, ammonium ions.

So then I wrote a memorandum to Hoagland pointing it out. At that time I thought it would work only on acid soils--that this could work on all the areas in the humid climates which have acid soils, and this method would have many advantages compared to the

Jenny: conventional methods. Soon afterwards we found that in neutral soils, as well as alkaline soils, the NH_3 was held in adsorptive form and was soon nitrified. So it would also work in non-irrigated arid soils. So it was a broad thing.

What was needed was a tool or an implement to put that ammonia gas into the soil in such a manner that it would not escape into the atmosphere. Evaporation of NH_3 was one of the problems with the liquid NH_3 fertilization. Agronomists claimed that as the water was flowing in the furrows, the ammonia would evaporate. So the idea was to have a nozzle to go underground--I didn't know how deep--that would put the ammonia in the soil and the plow would cover it up. A high portion should stay there, be absorbed, and there would be a higher crop return, a higher efficiency.

I wrote all this up in a memorandum and described what such an implement would look like with a gas tank and a plow and with a nozzle going down in front of the plowshare. But Hoagland--I remember I went in to talk to him about it--he smiled and was appreciative but he said, "This project opens a can of worms, so to speak, and this should not be done in his department." I could see that, although I said the university could take out a patent. The patent would have to be an instrumentation, although later a patent was taken for the idea. I don't know whether such a patent is still valid or not. We missed out on that, on patenting the idea. Anyhow, I was not upset or sad. I saw Hoagland's points, and I had other things to do. The syllabus was in progress and I was thinking of a book on factors of soil formation, and so forth. So this was just one of these incidents.

Maher: Do you have a copy of the memorandum?

Jenny: I have a carbon copy of the memorandum. I don't know why I kept that. It is interesting, in view of what happened subsequently. But soon afterwards, I remember, I met Mr. [F.H.] Leavitt of Shell Chemical who was somehow responsible for keeping track of the research, because the company remained active in water-ammonia fertilization and wanted to know what was going on in soils so they could use the data. So we had perhaps once a month or every two or three months a luncheon to discuss data and I showed him what we did. I told him about these new ideas and gave him a copy of the memorandum.

Then later on--I don't know how much later--somebody told me that Davis staff people were performing experiments in orchards on introducing ammonia gas into the soil. I started to ask about it and later I found out that Leavitt had applied for a patent (1939) soon after we had talked to each other. Looking over the patent--I have the copy here (No. 2,285,932)--I looked it over before our discussion--so it is nearly word for word taken out of that memorandum, even the idea. He patented the idea that the ammonia

Jenny: reacted to hydrogen and that that was a new thing, and also, too, he showed the tank--the picture of the tank--the plowshare, and the nozzle.

Maher: Was he a chemist?

Jenny: I don't know what he was. No, he was not a chemist. He must have been an engineer. So, of course, from then on it was like a tornado that went all over because it was so useful and so easy to do. The usefulness was in that the company would provide the application. The farmer merely had to call up to fertilize his field, and he had nothing to do with it. The company had all the equipment, the tanks and everything. The field results were very good, and application spread, and in that way I got involved with the company here.

Maher: Chevron?

Jenny: The Chevron Company. All over the United States they started to build plants to make ammonia. It became a big business. To me it was nice to know that a theoretical study led to a profound practical application, but I have no misgivings and so that was great. I myself had decided not to pursue it, so it was all right with me. I find it an interesting story which is nowhere told, and it's a case where criticism of agricultural research as being too theoretical could be opposed by showing that there are connections between theory and application. Of course, later other people made claims for the ammonia scheme. I remember Bradfield told me afterwards that when he was in Missouri he once had a clay which he titrated with ammonium hydroxide instead of potassium hydroxide. He thought that was an important thing, but I don't remember it. Maybe it had influenced me. I don't know that. How are you going to untangle all of these strands of influence?

Then afterwards, [Israel] Cornet worked on the reaction as a doctoral thesis, and he became a professor in the engineering department working on corrosion. He is retired now. He did a nice study. We had this picture that the NH_3 molecule would hook up to the exchangeable hydrogen. When the idea came that clays are aluminum clays, that became more complicated and other people developed different theories like [M.M.] Mortland at Michigan State. The crucial thing was, however, that the NH_3 molecule becomes an NH_4^+ ion without creating acidity, in fact, destroying existing acidity.

Maher: As you saw the expansion of usage of the direct gas application, was there an additional advantage in that this enabled an efficient application of fertilizer in dry-land areas as opposed to irrigated-crop areas?

Jenny: It spread all over. Since the dry-land areas have a larger extent than the irrigated areas, so more absolute tonnage might be used on dry-land areas.

Maher: If fertilizer is applied like some nitrate salt is applied in a semi-arid area or in a dry-land farming area, is there ever any difficulty in getting the fertilizer down into the soil because of precipitation not being great enough at any one time?

Jenny: Well, they would have to apply the fertilizer before the rains, not after the rains. The danger is, of course, that when you apply ammonium sulphate and the soil is calcarious, ammonia is expelled. You can actually smell it, when it comes off the soil.

Maher: Was one of the major, overall gains in this type of application from the fact that there was a greater percentage of the weight of material being transported all around that was actual nitrogen?

Jenny: Yes, that is always emphasized because one of the great costs of fertilizer techniques is transportation of the material, and with NH_3 gas that is much easier. Now, I understand, they have pipelines like oil lines for transporting it. But farmers tell me that the most important thing to them still is that they don't have to do the work. It is done by experts much better than they could do it.

Maher: Did you have any idea of the proportion of the gas that was typically lost from this application?

Jenny: By this application? I don't know of any experiments. I know that Schoonover made experiments on the evaporation of ammonia from irrigation waters and he showed that it is a significant thing. I don't know of any experiments. Probably there are, but I don't know them because at that point the patent was out. This was a closed chapter for me until another phase showed up with fertilization. That's another story.

Impact of World War II on the Department

Maher: What would be your general impression about how the Second World War rearranged or otherwise influenced academic, scientific progress at Berkeley?

Jenny: During the war I was head of the department. At that time department heads had meetings in the College of Agriculture once a week to plan for the postwar period. The assistant dean, Stanley Freeborn, who had to deal with instruction and all that, was in the navy. So we had planned what should be done in terms of research and teaching stating this in broad outline form. I remember that when Freeborn came back he just put everything under the table and started his <u>own</u> scheme. He probably got a lot of ideas during his war work. So all of these efforts had been, I would say, in vain.

Jenny: After the war, and especially during the war, the students who came were excellent. First, during the war they were the best because if they got low grades they had to go to the front. So they were working hard. Afterwards, when the veterans came for an education, they were interested in learning something. So it was a golden period for teaching and research. I feel it was very good.

Maher: How about the shuffling of relationships between departments?

Jenny: The emphasis shifted on the issues that came up.

Maher: Could you be more specific?

Jenny: As after World War I, there was again a move to settle veterans on agricultural lands, and thus there was more demand for soil surveys than was usual. It was in the Imperial Valley, on the Imperial Mesa, where politicians proposed these regions as such areas. So our department had to make general surveys and here and there spot surveys. That was emphasized in our department.

Maher: I think we can stop here. [tape interruption]

Jenny: I edited some events out because they were unfavorable experiences or home experiences that I tried to forget. So I have to really shake myself to get them recalled. I do that while I'm writing down notes. But there are emotional blocks of talking about some of the periods that I don't even remember.

Maher: That's understandable.

Association with Roy Overstreet

[Interview 9: June 11, 1980]##

Jenny: I think we should have something about my association with Roy Overstreet. I'm speaking now about the pedology, the state factors, and the way it was developing. That should take at least an hour. Then somewhere I will discuss the university and how soil science fits into it.

Maher: Dr. Jenny, you pointed out that Dr. Overstreet was working with you from the beginning--that he was helping you in your early colloid chemistry course. Can you give a picture of your relationship with Overstreet and what he was like intellectually and personally?

Jenny: The moment I got to Berkeley Overstreet talked to me. He was working with [James] Martin on fixation of potassium in soils and I gathered that he was not especially happy with the perspectives, the

Jenny: scope, and the intellectual challenge in that project. He had just received his Ph.D. in thermodynamics with the famous chemist, William Giauque. So he had broad views about chemistry and its application to other fields. Once, he also had a job in Hawaii as a chemist at one of the industrial companies there, but he came back to get his Ph.D. After I gave my first lecture and--having heard my views at my seminar on ion exchange the year before--he was interested in the approach I had. So we had a liking for each other--in terms of interest and also perhaps behavior. He was a rather reserved fellow who would talk in a low voice, but he made statements here and there that impressed me. So we discussed things and ideas together and he expressed a desire to work with me. No arrangements were made for me for assistants and he indicated he would like to play that role, and that's the way it came about.

Now, over the years I learned that he was, in a way, a strange fellow, certainly strange in respect to what we might call establishment go-getters. He spent several years at an art institute in Oakland, and then he went into chemistry. And throughout his life he never knew for sure whether he should devote more time to art or more time to chemistry, as he once told me. We used to go to art museums together, and whenever we went to meetings in Chicago, or Washington, or wherever, we would spend half a day at art museums because we had these common interests.

In his later years he did wood sculpture and we have a collection at home here. These sculptures are strange wood prisms and quite symbolic. I always wondered whether they had hidden sexual connotations in them.

Roy was a thermodynamic purist. At least that's what we called him and he liked that idea. He strictly based his work on classical chemical thermodynamics. He was symbol-preferring, doing everything by way of "A+B=C+D". The way I learned from him, was by accepting his critical attitude about the things I said, formulated, or stated, especially in connection with soil forming factors. He was very helpful there, not by offering ideas but by questioning and challenging what I had to say. In many ways my approach was intuitive, spontaneous, and impressionistic and he would pin me down in terms of logical rigidity. So I owe a great deal to him.

He was also a superb chemical analyst and, interestingly enough, in the analytical work we did together, he would never run duplicates. He thought there was no point in that. He would ask, "Which one of the two results are you going to take to get a mean?" He said he would have to take triplicates, at least as a minimum. But his skill and carefulness in analytical work inspired great confidence. I always had absolute faith in the figures he produced. He was opposed to statistics used to get reliability because as he said, it involved doing a lot of sloppy replications.

Maher: How did you respond to that?

Jenny: Well, I was impressed with the figures he came up with. Some I did myself and he was just right. I don't do that myself. I run several samples. I don't trust one sample. But he had this confidence. Also, I always was impressed with the way he wrote papers. He never wrote a draft or first sketch--as he told me. He just sat down with a pencil and wrote out in final and excellent form what he had to say. But, I did not see him do it. He told me he took advanced composition on the campus. Later on, after he died, I went through his office and found a lot of books on style, on English composition, and all that. He kept up and really polished himself in English; so it wasn't what it appeared to be, that he just had it in himself and it came out of his sleeve.

Maher: But he did formulate everything, going over it thoroughly in his mind before sitting down to actually write it out? Was that the case?

Jenny: That's what he implied, but I'm not sure now how it was done. But I know he never changed anything, even if I argued with him left and right, he wouldn't change a thing. That was all solid, just final and well written. He also, of course, was very critical in some ways of the establishment.

He was ascetic in a way, and later his closest associate turned out to be his former student, [C.] Krishnamoorthy, who was an Indian, a Gandhi follower, and they shared many of these ideas. We discussed a lot of philosophy. Sometimes we wondered about our problems with the contact theory, saying "What are we doing this for?" or "What are we doing research for?" We concluded that we were having a good time while doing research and that was the only reward we should expect. Anything else is just additional, whether it was praised, ignored, or ridiculed. Only the moment counted, and that was our conclusion about what we were doing.

Roy sort of left the impression as if he were a kind man, a modest man. But over the years I questioned that; I felt he buried a lot in himself. He did not complain, but he craved recognition. I could see that, although he implied he didn't. Maybe he didn't know it himself. I don't know.

Maher: In your discussions with Overstreet and Krishnamoorthy, how did you reconcile yourselves to the approach that the satisfaction of doing something well at the moment was an end in itself?

Jenny: I think he had not sorted it out, and he edited it out. I would say he had a lot of repressed emotions--and for years. For example, he and Babcock, his principle student, would always stick together and always go for lunch together, just like two isolated people. I know

Jenny: that once Dean Hutchison made a comment about it, and also other professors, that a university cannot afford to have too many such people.

Maher: What did they mean?

Jenny: Because Roy was reserved. He was not active. Roy did not participate in the life of the university. He would read, or draw, or do sculpture, or write, or study Greek, or deal with equations. He reminded me of a lone hermit who would live a life in a small cell and contemplate his world.

Maher: You mentioned that he was talented and cared for art and that this was evident in his own sculpture and the abstract art that he favored.

Jenny: I would not say that was all he cared about. He also like the youth style of the 1900s. I don't think he was confined to a period. He encouraged me very much when I was working on my story about soils as an object of art. He liked that. We had another fellow in the department, Professor Bodman, who was also interested in art, and after he retired he did a lot of sketching and had exhibits. Bodman and I used to go down to the harbor to sketch. But Overstreet and I never went out together sketching nature. He would draw and paint things at home and show them to me and discuss them. So we really had a good time together while it lasted.

Maher: With his thermodynamics background coupled with his tendency, at least as he said, to have everything in his mind prepared and then write out a text all intact--were those symptoms or manifestations of a tendency on his part to try to comprehend everything by the mind, to try to abstract through direct use of his mind?

Jenny: He was very much interested, I would say, in the broader perspectives and the broader consequences of whatever we did, especially, say, with the contact theory, and he liked very much the work I did when I wrote the book on soil forming factors. It appealed to him to have this whole complicated nature sort of symbolized into a scheme. He liked that. When the book came out he said, "That's the kind of book I like."

Also, he was very good at preparing experiments. For example, compared to Hoagland, our method was quite different. Roy described Hoagland as a researcher who pokes a plant and observes how the plant responds. That was Hoagland's approach, he said. Our approach was first to make a bet--a beer or bottle of wine--on what will be the result. In other words, we always operated with theories or with models and the experiment was always planned such that we would say to each other, "Now, what are you predicting?," if we do this or that, what is the result and why? So this tactic was elaborated at great length before we started the actual experiment,

Jenny: and after we had exhausted all these possibilities, then we would run the experiment. Someone who once sat in on these discussions (I've forgotten who it was), said, "You two guys are always trying to prove what you think," which is the opposite of the "poking" experiment and which is called biased by many people, and they think the "poking" one is more scientific or objective. Well, I don't believe that. I myself have always believed in concepts and models and systems, and I always thought that facts by themselves have no meaning unless they are connected to a theory or an hypothesis. Then facts become interesting. Otherwise, they are boring, unless they make money for somebody.

Maher: Then he expressed kind of a yearning for solving things that pertained to basic principles.

Jenny: Yes, I would say that his training in thermodynamics--and the whole chemical school he came from was perhaps limited, and he was greatly concerned that in the G.N. Lewis school of thermodynamics he never heard anything about the Donnan system, or about colloidal systems. He said, "They gave me no help. How am I going to solve these things?" So he spent a lot of time trying to bridge the gaps and get the Donnan system incorporated into his knowledge of classical thermodynamics.

Maher: Was he then attracted to soil science generally because soil science confronted the more general thermodynamics with basic aspects that needed to be incorporated?

Jenny: I don't think that he was basically interested in soils, even though he had a farm background. He very seldom came on field trips. No, soils was more an object to which to apply logic and the kind of training he had in thermodynamics thinking or analytical thinking.

Maher: That would be in the direction of applying the thermodynamic thinking to the field of soil science, not so much the other way of taking the problems presented by soil science and forcing an assimilation by the more general science?

Jenny: No, I won't rule out that with the contact theory he was really interested in what was happening. He wanted to understand that, so I cannot divide up his interests into black and white, or something like that.

Joint Work on the Contact Theory

Maher: I'm questioning you about this as background to your and his interest in the contact theory. I'm trying to understand why he would have chosen or been drawn to that particular problem. Can you explain how you and he were drawn to the problem of contact theory and what basic significance it had?

Jenny: The contact theory had its roots in Missouri, but it was vague. When I came to Berkeley and I met Kelley here, I told him what I thought these Missouri experiments meant.

Maher: That was what experiments?

Jenny: The experiments with Eric Winters, where we put iron minerals on hydrogen clay jelly and found that iron was diffusing into the jelly and evidently it wasn't diffusing in solution. All that time, I always thought what kind of picture should one develop to understand this and I used to talk to Kelley but it was a blank. But very soon I connected it with the electric diffused double layer of clay particles which connects with the exchange reaction and Overstreet liked that. It was something he hadn't known. The double layer had been clarified by Helmholtz toward the end of last century, and by other people. It was a challenge to physical chemistry.

So he liked that and he accepted my talking about how it might have consequences especially on surface diffusion. We started to run experiments by sticking nails into hydrogen clay (what we thought was hydrogen clay) and experimenting with it, and he was very enthusiastic. I remember his wife told my wife how happy Overstreet was working with these things.

What came really somewhat later was the application of the contact theory to plant nutrition. That was really one of many facets of the contact theory but later it became the overwhelming one in terms of the experimental work, but originally this wasn't the main aspect.

Maher: In this original period, why do you think Overstreet was so particularly happy to the extent that his wife would have noticed it so much?

Jenny: He had in me a companion who appreciated him and participated in his ideas and speculation, and I also appreciated his ideas in art, and about what he read. He was fond of Aldous Huxley. He was an early reader of a fellow who became popular in the sixties, a German writer who finally lived in Switzerland.

Maher: Hesse?

Jenny: Yes, Hermann Hesse, he read him, but especially Aldous Huxley--Point Counterpoint. He liked that very much. He always discussed these things, all sorts of theories, and I was happy myself working with him.

I don't remember exactly how it came that we applied the contact theory to plant nutrition. It was a logical thing being in a plant nutrition department. I don't know how much he thought about it, or initiated it, although I thought I had more to do with it, because I remember that way back in Zurich I was interested to know how roots take up ions, and in Missouri, [E.W.] Cowan and I published an experiment in Science on plants growing in calcium clay; they exchanged calcium with hydrogen. So I think I was really the pusher in this thing.

I remember when I first visited Berkeley and talked with Hoagland informally about plant nutrition and mentioned this experiment we did at Missouri with calcium clay, he said, "Yes, but we have different ideas about these things." I guess this encouraged me to start thinking about it and finally come up with the experiment of contact depletion with roots in acid clay.

Maher: Do you mean when he said that they have different ideas about it here, that was a challenge to you to try to reconcile the two? Is that what you meant?

Jenny: I believe it could be looked at that way because then he already was firmly impressed by the metabolic nature of ion uptake, whereas the contact theory was a non-metabolic step. So I think that was the main difference. But I wasn't fully aware of that at that time.

Maher: What were the next steps?

Jenny: We worked on experiments that you might call inorganic experiments on interaction of particles. I've forgotten when we started with an experiment with plants. We realized that in the uptake of ions it would be very difficult to distinguish between ions taken up by the root from solution or directly from the surface of the clay, because you could always argue that the ion on the clay surface hops first into the water and then goes to the root, rather than that it jumps across the gap. All we really said was that it could go directly; it is possible. We could never say that the ion wouldn't first dive into the water and then go onto the other surface. What was crucial, however, was really the definition of the soil solution. We soon realized that this was not clear. Burd and [James C.] Martin had done a lot of work on displacing water from the soil and they called that extract the soil solution.

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Jenny: Plant physiologists talked about the soil solution. Many had in mind the whole liquid phase, even the moisture which was very close to the particles; but, the displaced soil solution is a different thing. Finally, Overstreet and I had to iron this out, and I think he took the lead in that by stating that the soil solution is an extract or a displaced solution and is not necessarily all of the water that is in the soil. That was a clear distinction. That was an operational definition. But most people who do not know enough about soil moisture--the physics of soil moisture--or of colloid chemistry, don't make that distinction and they think that all the soil water is the soil solution, not only what you can displace by an operation.

I would say that much of the controversy on contact is based on a loose using of the term soil solution. So we stuck with Burd's idea of the soil solution, or with any others that displace it by pressure or suction. A considerable amount of soil water is not displaced that way. So this was an important thing, but I know we never could convince Hoagland to think in these strict terms. It is an arbitrary but an operational soil-water boundary. You see, the line where experimentation ends and concepts and pictures begin is not always easily seen.

So I figured out a new attack by having a gel of hydrogen clay and putting a root into it. The hydrogen clay might exchange rapidly with ions of the root surface. We knew that in this case you could not argue that the hydrogen ion on the clay would first go into solution and then lodge on the root. This had to be really direct interaction.

Maher: Why would it logically have to be direct?

Jenny: For a potassium ion on any kind of surface to go into solution and eventually onto the clay it would have to exchange first with a hydrogen ion of the solution and you would get potassium hydroxide in solution. But, if that reaction is small, yet interaction with clay turns out to be large, the hefty replacement would have to involve the two surfaces directly. We did that experiment with membranes and with inorganic systems and it was very clear. With living roots, of course, you couldn't be sure if and how a plant might deliver an ion into solution. So that's what we did.

We had a hydrogen clay. We put an excised root in it, a root which had been starved of nutrients so they would not flow out of the root by leakage or reaction with water. So we had first to show that for a number of hours (say, ten hours), the "low-salt" root in water lost but small amounts of potassium. That was possible with "low potassium" roots. When we put the hydrogen clay in contact with the root, then copious amounts of potassium came out of the root. It was a startling thing.

Jenny: I remember we were so curious what the test would show. Overstreet analyzed the roots for potassium before and after immersion into clay suspensions. We were not waiting for the final weighings. We were just staring at the potassium precipitates. Are they different? Right away we knew from the volumes of the precipitate, that it was true; contact had operated. Afterwards, we did a lot of work on what we called "contact depletion" and we always felt this was a clear indication of particle/root interaction. But very few people, as far as I know, have ever repeated the contact depletion experiments. They all went in for contact uptake experiments.

Maher: Why do you think they went toward that?

Jenny: For one thing, depletion depends on how plants are grown. When plants are grown in a Hoagland solution--and that's what most people do--then the plants are full of ions and there is always heavy leakage. But what we did, we grew plants in a half-strength Hoagland solution and just kept them growing until they had taken up everything, and then we starved them. So in our thinking, all this potassium went into non-leaking form. There was no oozing out of ions unless you induced contact. You need this extremely low nutrient status of the root.

The Use of Radiation in Soils Experiments

Maher: When, in the course of the development of these experiments, did you go into the use of the Geiger counter and radioactive potassium? Was it from the beginning?

Jenny: No, at the beginning we worked with ordinary isotopes and a mixture of isotopes and ordinary chemical analysis, which meant we had to have large amounts of roots. Then we became interested in working with intact plants, not excised roots. We wanted single plants, and with it came the problem of accuracy, of refined analytical techniques. At that time, the radiation laboratory had started to produce radioactive elements. Right away we were in it and got radioactive potassium and other elements and used them for ion uptake or ion depletion by measuring counts.

In those days, you just got a bottle of solution which had been treated by the cyclotron. Then you had to do all the purifying yourself, and the separating of elements, and all that. Overstreet was very good at that and Stout helped. Then we had to have a Geiger counter, and Stout constructed one in our department. Before that, Stout had qualified himself while making a living as a graduate student by constructing glass electrodes and pH meters. That's the way he supported himself. He was a physicist and he had the necessary background and he made these Geiger counters; but

Jenny: nothing was automatic. So it was very slow, and often we had to work all night because the potassium was decaying rapidly and there was nothing automatic. But it worked. We got the information all right.

Maher: What was the year, the time?

Jenny: It was, I guess, 1937 and '38.

Maher: How did you come into contact with the prospect of using the radioactive potassium? Did you know somebody in physics or was it just that everybody knew what was going on?

Jenny: For one thing, everybody knew what was going on. I remember a lecture given by [J. Robert] Oppenheimer where he actually drank a glass of water with radioactive sodium chloride in it, and then showed two or three minutes later that his fingers were radioactive. I guess he wouldn't do that anymore, but at that time he did. So this was known.

Hoagland had the connections. Hoagland was an important man in the budget committee. So I'm sure he made the connection. Overstreet was known in the chemistry department, so we had no problem. We got the stuff and really on a preferred basis.

I think we did the first plant experiments with radioactive compounds, except for the work done by [G.] von Hevesy several years earlier, with lead, with natural radioactive lead compounds. But afterwards, use of radioactive isotopes spread like wildfire. Everybody used them.

Maher: Did you known [Ernest] Lawrence or was it Overstreet who knew him?

Jenny: Well, we both knew Lawrence and had talked to him. There was another reason why we had contact with Lawrence. That was that the first cyclotron was built by a graduate student who became a soil physicist. The cyclotron was a small apparatus. He became an outstanding soil physicist and worked a lot with Bodman. Oh, his name is so familiar. Anyhow, I don't know it right now. [Niels E. Edlefsen] So there was another contact. So "soils" was known, at that time, to be very progressive through the colloid chemistry and this thing with Bodman, and that student, and Hoagland. Stout, of course, was there; he right away ran experiments with radioactive substances. So it was an exciting period.

Jenny: We used to walk around with long sticks carrying little containers with the radioactive stuff in them. The moment a person walked through the corridor with his stick the Geiger counter in the lab started to rattle, and we knew that something was going on. We had very little notion that this was dangerous or could cause cancer or anything. Overstreet developed skin cancer. I developed skin cancer in 1947. I don't know whether this was related to our experiments or not.

Maher: When you first published the results of the depletion experiments, do you think that you were able at the time to differentiate between excitement over the meaning of the experiment itself and excitement over the usage of radioactive material in a biological experiment?

Jenny: I would put it that we were excited about the contact effect and were highly pleased at having these radioactive isotopes to operate on a scale not possible before--working with isolated roots or single roots. This helped clarify the point.

Maher: So that was your reaction; that is, yours and Overstreet's and the faculty around you. What about the reaction of the national audience?

Jenny: They were greatly impressed by the use of the radioactive substances. I remember when we sent the manuscript to Soil Science in New Jersey. We got a nice letter from the editor, Dean Lipman (Director Lipman), saying how he appreciated that we submitted the material because that added a lot of prestige to his journal.

However, I should say, in respect to your previous question, for us, really, the use of radioactive isotopes was not crucial for the contact theory, whereas what Stout did--studying the migration or flow of phosphorous or potassium in the plant phloem and the xylem--that could not have been done otherwise. So there it was more crucial that the radioactive isotopes were available. In our case, it made it more convenient and nicer to work with.

Maher: I'm still trying to pin down the answer to the question on the reception by the national audience, including Lipman. Can you distinguish between their excitement over the experiment itself and their excitement over this being a novel usage of radioactive material?

Jenny: Say, by the people at Cornell, or the people at Wisconsin?

Maher: Yes, or by Lipman himself?

Jenny: I don't know. I can't answer that. I don't know that. Both things were new, so it was something like two independent events coming together, and you get a squared effect. So it was impressive. But you see, Berkeley was really a terrific place in those days--

Jenny: radioactivity and thermodynamics--it was a wondrous place. Ours was a great show. A lot of people started to draw conclusions, which we never did. For example, Schoonover, the soil extension specialist--I heard him talk to farmers saying that this was going to revolutionize agriculture. Well, we never thought so. People exaggerated as if, you might say, a Messiah was showing up or something. I guess people react that way. And I can understand an extension soil worker, who is anxious to have some good news for the farmers--that he would tell his constituency an exaggerated story.

Maher: How would this have been good news to the farmer if it was exaggerated at that time?

Jenny: Looking back now I don't know what Schoonover had in mind. I don't know how much he really understood about what was involved. It had one aspect that we talked about already, namely that contact offered an opportunity for using fertilizers that are not very soluble. If they were finely divided and mixed through the soil, the roots could utilize them. So maybe he had that in mind. I know we talked about that. Later on, we actually ran a pot test experiment.

Maher: Do you think it did work out?

Jenny: Well, I was just thinking now, because when we used it--as we may discuss in the section of pot tests--we used an insoluble phosphate fertilizer which was finely divided, and we mixed it with the soil and put the soil in the pot, and the plant utilized it very well. But when it was applied in the field, just plowed under very shallowly, it wouldn't work and the result was poor. We assumed that by incorporating and mixing the fertilizer in the pot, we created a more favorable phosphate source than in the case of ordinary surface application. But this was not explored. I cannot answer that.

The Ultramicroscope and Ion Movement Models

Maher: In much of the Missouri work that you had done, as exemplified specifically by your work with potassium and sodium, and more generally by your paper on the model for ion exchange oscillatory movement, now manifesting itself again with contact theory and the migration of ions on the surface, and in contrast with the emphasis, especially later, in ion exchange on the thermodynamic approach you have tended--it seems to me--to concentrate a great deal on trying to envision or model what was occurring at the surface with ions. I've always wondered in reaction to that, whether your earlier experience as a very competent user of the ultramicroscope--having such a close exposure to the ultramicroscope and the vivid Brownian motion of small particles--whether that may have been a guide to

Maher: you to continually try to envision the actual occurrence at the surface. What went on in your mind when you viewed these situations? Were you constantly seeking for something as if you could be down there looking at it?

Jenny: Well, this is an interesting point. I would have to-- It didn't occur to me, but what you are pointing out may be valid. As we discussed once before, the ultramicroscope--that is, observing moving particles--was an aid in accepting the atomic theory. So you could say there is a logical connection between the ultramicroscope showing the particles moving and the ions at the still lower level of magnitude moving around.

When I heard the first lecture by Debye on the activity theory, he talked about these ions moving around and touching each other and making couples, and whatnot. So my whole upbringing was in terms of models. The whole emphasis was that, really, this kinetic view is in a way more stimulating and fruitful than the rigid thermodynamic approach. So I guess I just followed this along. Maybe it also has to do with my interest in nature and drawing and seeing things, a visual thing. A visual thing had always been better to me than hearing. My musical education was not zero but not advanced. So vision has always been with me the foremost sense impression.

Maher: Do you remember an impression of maybe your first visual experience with the ultramicroscope, seeing the movement? Did it strike you in any way?

Jenny: I couldn't place the instance, but it was a very interesting thing to see these particles move around. It was a mysterious thing. So your comment is interesting, that this may be related to my search for mechanisms. I remember as a chemistry student, to be a good chemist you had to memorize the atomic weights. Then later, in Missouri, it was really more important to know the atomic radii, and so you memorize those because now you have got the crystals and everything was "visible," especially when you deal with solids; in colloid chemistry and surface reactions the mental visibility was helpful.

More on the Contact Theory, and its Place in Soil Science

Maher: Now, getting back to this early period in the contact theory, was your investigation more out of an interest from the plant nutrition aspects, or as an interest in what occurs at surfaces?

Jenny: I would say, the first interest was in the clay surfaces in relation to soil formation, on what chemical reactions go on in the soil. The plant side of the contact theory came up when I was with

Jenny: Hoagland and when he started to question some of the ideas I had on plants and clays, and that started me thinking about contact with roots. To me, the big thing was to be able to say that plant roots can take up ions without having to rely on ions in solution. That I thought was fun. That was interesting to me. Extracting--making water extracts--that was only one aspect. Contact was more complicated, and it made a continuity of root particles and soil minerals possible, which I appreciated. If an ion that oscillates is considered part of the solid, which depends on the definition--how you separate the liquid phase--then a root double layer joining a mineral double layer would be a unison of two surfaces. So they would be together, and I liked that idea of not separating plant and microbes and soil, the organic and the inorganic world, as rigidly as it is done.

Maher: What was the reaction of the scientific world to the contact theory?

Jenny: It was very favorably received as something new and we were invited, for example, to a national meeting of the American Chemical Society at Stanford, and we received interesting and favorable comments. No, people accepted it. I remember we gave a seminar in life sciences--

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Jenny: We had close contact with our botany department and also with the botany and zoology departments at Stanford, where they discussed ion movements; we had lots of private seminars, so to speak, on the campus with various people.

Maher: But the depletion experiments were not repeated to your knowledge?

Jenny: We repeated them, but I never saw others do them, no. Plant physiologists would say, including [Louis] Jacobson, "We really don't know what is going on." That may be right.

We probably picked the most complicated situation to work on contact phenomena--by working with clay soils and plants--because really the plant picture was not fully understood. All sorts of ideas and theories had to be considered, rejected, or disproved, and this was not an easy thing to do and get consent among other people. You see the same problem right now in the latest book on diffusion of ions in soils by [P.H.] Nye where he discusses critically the contact theory and says, "Maybe the ion is reacting with water before it makes the next step."

What we proposed is a mechanism. Other people would say--as [P.F.] Low at Purdue University says--"contact is really nothing but diffusion." Yes, it is nothing but diffusion but it is a model of diffusion, namely how ions hop around. Diffusion equations don't

Jenny: have a mechanism model. They merely describe how the substance moves ahead. So it has a lot to do with personal background and with definitions.

Further, if you put a plant root in a solution, say, of a radioactive substance, an iodide or a potassium salt, for just a second, and pull it out and wash it, there is still radioactivity on the plant. That was then considered ion uptake, though the ion may not yet be inside the plant. It may still be on the surface, but it was considered ion uptake, a first step, because the root surface is part of the plant. But later on, in the 1940s, with [E.] Epstein in the picture, more and more the term ion uptake was restricted to metabolic uptake. That means the ions had to be inside of the cytoplasm, someplace in non-exchangeable form. The exchangeable ions, as in the cell walls and on the pectins and so forth, were really not considered anymore. So in that way there was a loss of interest in the contact picture.

But I feel as ion uptake from soils is studied further, this will come back again because, after all, these surface reactions occur, and the question about how important they are has to be figured out. In recent years at least, Jacobson and [M.G.] Volz have come out with the idea that the exchange capacity of the roots is important for the metabolic uptake. So there is again a link. So I'm confident that in some form or other, these ideas will persist.

Maher: You mentioned in your write-up here, articles by M. Peech and J.V. Lagerwerff.

Jenny: Yes, they claimed they had disproved the contact theory with experiments. But it was later pointed out by the people in Ohio--by R.E. Franklin, for example--that actually the Cornell team did <u>not</u> perform the experiments the way we performed them. They added nutrients to the solution to keep the plant growing and thereby the contact effects were masked. In their publication this wasn't stated. The people of Ohio ferreted it out and showed that the experiment was not valid. Lagerwerf finally admitted that he had found a contact effect experimentally and stated it in a paper. But after that he never touched the subject again.

Maher: Do you think there was an emotional reaction afoot somehow?

Jenny: Emotional? There may be competition between institutions, but not anymore, I would say. I would say the people don't think in terms of contact, or don't use it. I don't think there is any personality issue involved. That was maybe twenty years or thirty years back. But now it is more a matter of interest in what phase of ion uptake is being studied, and is contact helpful or not. Whether or not a theory is helpful depends on your own ideas, what you think about a

Jenny: certain phenomenon. I would say right now it is just sitting and waiting--like an object in a store room--to be appreciated again, or recognized for what it is.

Ion Exchange Equations

Maher: Let's backtrack a bit in terms of time and shift over in terms of subject matter to the ion exchange equations. You developed this material in our talk about the Missouri period and it underwent further development, I understand, when you went to Riverside and then to Berkeley. Could you go over that path?

Jenny: In Zurich I grew up, so to speak, under the Freundlich equation, which is a mathematical formula whose equation gives a curve that has no maximum. It was developed for the adsorption of gasses on solids. The more gas is added to a solid, the more gas is taken up by the solid. Experiments showed that no maximum is reached. Adsorption keeps going up and up as more gas is applied. With fertilizer adsorption or salt adsorption by soils--in many soils the same thing was observed. It kept going up and up and no maximum was reached. That was the justification for applying this Freundlich equation to ion exchange reactions. What we didn't know at that time, or what just was becoming known, was the presence of hydrogen ions on clay particles. That was the result of the improvement in techniques of measuring small quantities of hydrogen ions. The pH concept had just been developed in Denmark by Sörensen

When I was in Paris, Wiegner sent me a publication from Holland by J. Van der Spek and he wrote, "This paper pretends that there are hydrogen ions on the clay particles." The paper had been written in Dutch. I didn't know the language, but I got a dictionary and was able to translate it and send it back to him. Van der Spek stated that there are exchangeable hydrogen ions on clay particles which had not been recognized by analyzing the salt solutions. When these hydrogen ions are present, and you add KCl, then KCl is taken up but you might not find any calcium coming out. This discrepancy had been a very confusing thing for many decades.

With the recognition of hydrogen ions on clay particles, adsorption maxima were observed; that means the clay particles would take up a maximum amount of an ion and not more. I had found exchange maxima in my thesis in some of the neutral permutites, and found it especially when I got to Missouri and worked with Putnam clays. With increasing salt concentration, the adsorption went up and reached a plateau, and by adding large amounts of salt that plateau was not exceeded. A plateau is also contained in the Langmuir adsorption theory. Langmuir published an equation which

Jenny: had a maximum, in contrast with Freundlich. Langmuir's theory also was a gas adsorption theory, but it was based on the law of mass action. So these were the two competing models of ion exchange equations: the Freundlich equation and the Langmuir equation--the second one reaching a maximum, the first one not.

So, when I started with my picture, I postulated, on the basis of the crystal structure of the model I had, that a maximum would be reached, and I formulated equations so that the maximum was included in it. And it really then turned out to be a modification of the law of mass action, which has a maximum. I was not able to derive an equation that involved calcium or magnesium against potassium or sodium. All I had were exchanges between monovalent ions and exchanges between divalent ions, but not exchanges between monovalent against divalent ions, such as calcium and potassium.

When I got to Berkeley, and especially when I became head of the department, I was at the same time the head of the soils department at Davis, because the two departments were under one head and the head was stationed in Berkeley. Professor Lannes Davis was at Davis and I went there at least once a week. He became interested in my equation and we discussed it and I said it would be good if the problem of monovalent against divalent ions could be solved. So he worked on this and published an equation based on the same model I had of the oscillation volumes and came up with an equation. But when the equation was tested against experiments, it turned out that the constant of the equation was not a constant, which was a serious drawback.

Now, Davis and Overstreet had been together in Hawaii and knew each other. So Davis discussed the problem with Overstreet and got Overstreet interested in ion exchange equations. I told the two that at Riverside I had met [Albert] Vanselow who had published an ion exchange equation based on activities, also assuming a maximum, and that they should consult him also and what he had to say, which they did. They studied his paper and since Vanselow also was a product of the G.N. Lewis school at Berkeley, they used the same vocabulary of activities and thermodynamics. So Overstreet and Davis started to develop ion exchange equations based on the activity concept.

The real problem that Professor Davis had and which I had--and why I couldn't do it--was the question of the activity of adsorbed ions, especially of the activity of an adsorbed calcium ion which has two charges. How is this to be evaluated? What model must be made? We could not say that the activity was that of a solid because calcium was not strictly part of the clay particles. So it could not be just unit activity. So it had to have a special value and how is this to be done? That was discussed at great length between Overstreet and Davis and later with Krishnamoorthy and also

Jenny: with Ken Babcock, I think. They came up with a model of how to set up an equation and put in a term for the activity of the divalent ions, as calcium.

At the same time, a Russian soil chemist, Kapon, had published an equation involving monovalent and divalent ions which had been accepted by the people at the salinity lab in Riverside because the problem was important in connection with sodium ions against calcium ions. A good soil has calcium ions, a poor soil has sodium ions. So the transition between the two depended on sodium/calcium exchange and it was desirable to have an equation to formulate this exchange reaction. So Kapon's equation was also considered, but its derivation was not accepted because the constant wasn't really constant either.

So at Berkeley, Overstreet took the lead in developing a thermodynamic approach of ion exchange equations and set a pattern of, you might say, worldwide interest in ion exchange equations. That was continued by Ken Babcock and now is being continued by a third generation, so to speak, by [Gary] Sposito, who worked with Babcock and is now a professor at Riverside. But meanwhile people in Europe, especially in Holland, also started using thermodynamic equations and discussing activity coefficients of adsorbed ions. And at present I believe that the center of activity is in Europe. But I see no reason why it shouldn't switch back again to the United States and maybe to California.

Maher: What was your view as you watched these developments in the hands of Davis, Overstreet, and Babcock who were, as you say, talking in the language of G.N. Lewis, and were also very heavily versed in thermodynamics? At the time, did you see any transitions that were remarkable to you or that may have--

Jenny: The way I saw it, and the reason I did not participate in it (or one of the reasons), was that the thermodynamic approach--the way Overstreet talked--was essentially an aesthetic effort to formulate encompassing equations that satisfied certain logical demands. Now, the advantage I saw in my equation, which apparently Overstreet and his group did not consider important, was that my ion exchange constant related directly to other colloid chemical phenomena, namely it could be expressed as the ratio of the thickness of the double layers. Therefore, it could be linked to dispersion and flocculation.

To me the whole connection of these things was important. In the thermodynamic equation that was not considered; there was this ion exchange constant (the thermodynamic ion exchange constant) which could be put in other equations, say, of free energy changes, for example. But to other colloid chemical phenomena, there was no relation or none was shown up. So to me, this was not satisfactory enough. I was glad that they pushed these things and got equations,

Jenny: and I myself was not mathematically advanced enough to go into statistical mechanics or quantum mechanics to develop my own picture on a quantitative basis. I had other interests. So I was just an outsider and a pleased visitor observing what they were doing. And I still am.

I think it's great when they come up with these formulas and really come up with constants. Eventually the breakthrough will be to interpret what these constants mean and that is probably going to be a big thing if that is tackled. But I don't see the fellow yet who would do that. That's a great challenge.

The Junction Potential: An Unsettled Controversy

Maher: How did the question of the junction potential weave through this whole period?

Jenny: The question came up in as a result of the contact theory when we were using hydrogen clays and wanted to get an idea of how much hydrogen is in a double layer, and in connection with Overstreet's interest in activities. For example, does the glass electrode really measure activities of hydrogen ions on the particle? Is that measurement really the thing? We had shown that hydrogen ions pull out ions from the root, but the equilibrium solution would not. So there was something peculiar about the hydrogen ions on the particle. The question is, is the pH a valid measurement? Are the hydrogen electrode and the glass electrode really responding to these adsorbed hydrogen ions? So we started to work on this topic and found that we got a reading on the pH meter, not by the glass electrode touching the particles, but by the salt bridge touching them. That way we got an EMF, a voltage change. So this opened up something unexpected, which we found nowhere in the literature, because it was always assumed that the potential jump at the KCl bridge was negligible.

The observation also started a discussion of the "suspension effect," meaning that a soil paste has a different pH than its water extract, which Wiegner had called the suspension effect, and which had been explained early by [N.M.] Comber as a Donnan potential. Now, we found this effect of the KCl bridge. So that opened up the whole discussion: Is pH the result of the KCl bridge, of the Donnan potential, of a junction potential, or of exchangeable hydrogen ions?

After we published this, it created a great controversy and it has not yet been settled. If you look at books on soil chemistry, on soil pH, the suspension effect is mentioned and the explanation is given either as a Donnan potential and nothing is said about the junction potential, or the explanation is given as a junction

Jenny: potential without saying anything about the Donnan potential. [H.] van Olphen, or [R.G.] Gast in his latest chapter in Minerals in the Soil Environment, explains it as a Donnan potential, whereas the people in Holland in their book on soil chemistry strictly speak of the junction potential. It is an unresolved matter and it touches on what we mean by pH and how pH is measured. These are unresolved questions.

Maher: You state in your manuscript that after the Second World War, when you were returning to the question of this effect, Davis had commented, "Why dig up a dead horse?" What was your response to that?

Jenny: Davis had obtained his Ph.D. on this suspension effect with clays. And he felt he had explained it--I've forgotten how--and when he came to the lab one day and we told him that we were working on it, he said, "Why dig up a dead horse? I had it all worked out." But he was not aware of our experiment consisting of touching a large resin particle with the salt bridge.

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Maher: With respect to the Donnan potential and, on the other hand, in terms of junction potential, what is your reaction to the nonactivity in this and the avoidance?

Jenny: My reaction is, first, that there is a dualism which has not been resolved. We have here a natural phenomenon and investigators coming at it from different backgrounds, and with different systems of thinking, different models, and so we look at this phenomenon differently.

A lot of people still measure pHs and ion activities with all kinds of electrodes--potassium electrodes, sodium electrodes in colloidal systems--and I still believe their pH readings or activities readings have limited values.

Maher: In the past thirty years have you tended to raise questions about these inconsistencies, say, in seminars where you hear discussions?

Jenny: I might have and other people have. For example, [S.R.] Olsen at Fort Collins [Colorado]. He has recently gone into it and mentions the conflict. He tried to develop a measuring device for pH measurements in soils which are related to soil solution. He thinks that soil solution is the crucial pH you measure in the soil. I am leaning towards that myself and certainly if you go into ion exchange equation, which involves a pH value, I would think it would have to be a soil solution pH. But I have no reason to think any further, so I just let it sit. I cannot worry about all of these things and about Russia and the Middle East and all of these things. [laughter] It's impossible!

Maher: Is there anything else you would like to say at this juncture about Overstreet?

Jenny: [pause] No, I think I said most of the things that occurred to me at this time. I haven't been able to get hold of the obituary I wrote for him. Maybe I said things there which I haven't brought up. He wrote a famous paper with Louis Jacobson on the carrier theory of ion uptake by roots. But I've never been able to find out whether Roy had the idea or Jacobson. I can hardly believe that they both conceived it. They were close because during the war they worked together on a secret atomic energy project. So they had a lot of discussions and contacts. So I wouldn't know. But later on Overstreet changed his mind and published a paper on abandoning the carrier theory, or really saying the whole internal of a cell, the cytoplasm, is the carrier, which meant he really went back to surface migration.

Maher: When we spoke in the lab last week about the reaction to the contact theory and also the reaction to the junction potential (probably mostly the former), you were alluding to very strong feelings and almost vituperative reactions sometimes. Do you feel that you have adequately expressed the type of reaction that was given to your paper?

Jenny: No, there was Marshall. Marshall did a very good job of measuring membrane potentials, but what we had did not fit into his scheme. So he was quite upset and became quite aggressive and he was very unfriendly in his articles, denouncing the junction potential. But that is now passed. He visited me two or three years ago and said, "I haven't changed my mind," and I said, "I haven't changed my mind either." That was it and we had a good time together! [chuckles]

I should add one thing about the contact theory. In relation to plant nutrition, it has been brought into focus by [Stanley] Barber and his group at Purdue University and they claim they have results that are in favor of it. But you cannot absolutely say it's proven, because someone could cook up a different model. At least it is in harmony with it and a solution model doesn't explain it. Barber has developed the idea of distinguishing ion uptake by mass in the transpiration stream, and by diffusion of ions to the roots, both for ions in solution and also ions on clay particles by surface migration. He analyzes the plant for, say, potassium uptake. Then he measures the amount of transpiration during, say, three weeks of growth, and he knows the amount of potassium in soil solution. So he calculates that way the amount of potassium taken up by mass flow and the difference between the total potassium taken up and the mass flow he says is diffusion. He mentions ion exchange diffusion on surfaces. For the quantitative picture I have a reservation.

Jenny: He considers the plant root at rest and the ions moving to the root surface. But I could visualize that the plant root itself is "diffusing," namely advancing and coming to the exchangeable ions, and that would give the same effect. It would be a contact mechanism, even a better one than having to assume that the ions move to the roots by surface migration over a long distance.

Maher: It is an interesting observation. [tape interruption]

Jenny: The way I see it, all the plants in the world use it.

Maher: The contact theory?

Jenny: Yes, but I can see nothing in it that helps agriculture or helps the plant to improve its status. I should say (and maybe I said that before) once at the department Christmas party, everybody gave a little piece of entertainment. Babcock read a poem about plants dying all over the world because they couldn't make up their mind whether they should take up ions by solution or by contact. [chuckles] I'm still at that stage of not knowing.

The Mucigel Story

[Interview 10: June 18, 1980]##

Maher: Dr. Jenny, can you explain what connection there was, if any, between the work that you had done on the contact theory and the work that led into the discovery of what was termed the mucigel?

Jenny: Well, I was interested in the nutrition of plants growing in soils, which I felt is quite different from growing plants in water cultures that the plant nutrition people do. I visualized the possibility of interactions of the electric double layers of the root surface with the electric double layers of the clay particles. When a root forces its way through the soil, it pushes soil particles apart, and contact interactions between root and soil should be quite marked. What, I wanted to know, are the reactions going on and affecting plant nutrition in this contact zone.

We had to figure out models of what a root surface really looks like. In our early publications we sketched the root as a wedge adjacent to a rectangular clay particle. It was merely a symbolic way of presenting the interaction situation.

Now, my student friend, A. Frey-Wyssling in Zurich, had initiated a lot of root studies with the electron microscope. His interest centered on the cell wall, how it is structured with thread-like molecules.

Jenny: His observations intrigued me. The publications he sent me provided a detailed picture of what a root surface might look like. And these pictures, with the cellulose molecules and the pectic substances with their carboxyl groups, impressed me and induced me to study root surfaces in terms of ion exchange and related properties, as Nat Coleman and [D.E.] Williams were doing.

Maher: Were Coleman and Williams looking at this completely independently?

Jenny: No, they were working with me in the lab at Berkeley. They selected the ion exchange properties of the root as a special study.

Maher: What prompted them to make that study?

Jenny: Well, our general discussions about the contact theory as related to nutrition of plants in soils.

When departments in Hilgard Hall obtained in the 1950s an electron microscope I thought this new tool would be a good way of studying the root-soil boundary, and I got Dr. Karl Grossenbacher involved. He had graduated in plant nutrition and was out of a job. The project was financed by the Atomic Energy Commission.

The procedure involved growing a plant in soil, then quickly dehydrating soil and root together and imbedding the two in liquid resin that would harden and turn the sample, root, soil and resin, into a stone-hard body. Then with diamond saws and knives ultrathin slices were cut off and observed under the electron microscope. Dr. Grossenbacher was skillful and did the detailed work of preparing the thin sections and getting their photographs in the electron microscope.

My contribution was deciding what soil-plant combinations to study, what results to look for, and together we tried to interpret the photographs. We started with growing roots in tap water, and we got nice pictures of the inside of the root, the cytoplasm, the vacuole and the cell wall. But the outside region was hazy and there was no sharp root boundary. We knew from botanical literature that a thin layer outside the cell wall exists, the mucilaginous layer, but we could not see it on the photos or be sure whether or not it was there.

So, using colloid chemistry knowledge, we put the roots of plants into a suspension of colloidal iron oxide particles. We knew they are positively charged and we expected them to stick to the negative root surface and that's what they did. In the electron microscope the iron particles showed up beautifully, marking the boundary of the mucilage, the outer boundary of the root. And this mucilaginous layer of plants grown in water was much thicker than what had been reported in the literature. It was even thicker than the cell wall.

Jenny: Also we could see bacteria in the mucilage. That was a surprise because bacteria are supposed to be outside of the root in the so-called rhizosphere or rhizoplane, but evidently, in our case, we had instances where the bacteria were in the mucilage, inside the iron coating.

Maher: That was surprising to you? When the results were published, did anyone ever point out that that observation had been made earlier? That the bacteria might be in that region?

Jenny: You mean independent of what we did?

Maher: Yes.

Jenny: No, the mucilaginous layer was a specialty of a few plant anatomists, and I don't think they reported the presence of bacteria. When I took the pictures abroad, in 1963, microbiologists were quite impressed by what they saw. A microbiologist from Australia, Patrick, I believe was his name, came to my office, looked at the photos and said, "This mucilage zone would be the ideal location for nitrogen-fixing bacteria, because there they would get from the root interior a lot of nutrients and organic materials which they may need as an energy source to fix nitrogen from the air."

However, we were more concerned with the root boundary as it existed in the soil. First, we operated with an artificial soil material named permutite; it's an artificial aluminosilicate. It is relatively soft and can be sliced by the diamond knife. Soil with quartz crystals in it would be too hard, so we used permutite and got good pictures.

Maher: How did you get a positive charge on the permutite?

Jenny: No positive charge was needed.

With another technique we grew seedlings on a gel of montmorillonite clay that also could be sliced, and we got some very beautiful pictures showing the root surface and the montmorillonite crystals.

I should add that this work was tedious and required a lot of repetitions. In fact, of the hundreds of pictures we took, only a small number were suitable for publication. And even those did not come up to the standards that had already been established for electron micrograph pictures of organic materials. The big drawback was that in our work we had to impregnate soil and root with resin. Distortion of tissue was possible. We found cell walls compressed and things folded. We thought they were artifacts, and we never published them. But other workers that I know from Australia think that distortion is normal behavior, is the way tissue occurs in the soil, and that it isn't an artifact.

Jenny: Then, looking at these pictures, we find that the zone between the cell wall and the clay particles was a variable zone of contact, sites with and sites without contact of cell wall and clay. In the no-contact areas, the shading was darker and we identified that as mucilage material. But now it was no longer a layer, it filled the soil pores. And that's why we called this material the mucigel, because it wasn't a layer, it was a gel filling up the pores between the cell wall and the clay particles. So that's the way we used that name. It was a name of convenience.

We made lots of pictures and published some of them. But we had many more unpublished ones and some beautiful ones with colloidal gold particles, some large and some small, and you could see that the smaller gold particles penetrated deeper into the mucilaginous layer than the coarse particles, so the root surface was a fringed boundary zone.

We were also concerned about the properties of the mucigel, especially whether it had ion exchange capacity, and this we did with treatment with methylene blue, which is a dye, a cation dye, and we could see that the dye was fixed by the mucigel and was also exchangeable. So that convinced us that the mucigel had cation exchange properties.

There was one problem with Dr. Grossenbacher. He was to publish papers about his contribution to the research. When he finally did prepare something, it wasn't accepted by the journal he sent it to. He had some difficulty in writing and in the approach to writing. That was a disadvantage.

Quite a bit of work we did with mucigel never was published. One of the reasons was Grossenbacher's writing problem. The second was that, upon my retirement, the project was discontinued and he was again out of a job. Today there would not be much point in publishing these old picture because they don't come up to modern techniques.

Maher: Are the picture still about?

Jenny: Yes, the pictures still exist. I have them in my office.

It's very unfortunate that that's the way it ended. Also, I thought, looking over the subsequent papers which have been published by people in England, and France and the Australian workers, nobody repeated that experiment with the iron oxide coating the outside of a root grown in water. I would have liked to see confirmation by an independent source, but nobody did. I guess they're not familiar with the possibilities of the colloid chemistry of a root surface.

Jenny: Also, I should say, when we looked at the contact zone where the mucigel touched the clay particles, we speculated on the chemical interaction between the two. We knew, for example, that if we put a root into a suspension of montmorillonite clay, then took the root out, washed it and processed it, there were seen clay particles sticking to the surface of the root. Forces of attraction must have been operating between the two, and knowing that the mucigel had exchange capacity, presumably carboxyl groups, I postulated that they might hook up to the aluminum ions on the clay particles and therewith make a chemical bond.

And I liked that idea because it visualized a continuum from root to soil, and as I was interested in ecosystems that aspect was pleasing to me because it made a unity of the two systems, plant and soil, which are separate in the science hierarchy.

I'm interested in a book that has been published recently on the root-soil boundary, and the mucigel is given a prominent place. Incidentally, it's a book published in England, and I am wondering why it is that many of the soil research books, advanced books, come out of Europe and no longer originate in the United States--even the books we are using in classes. But that's a sideline.

Maher: Well, to continue that sideline just briefly, when do you feel you observed a shift back to European publications?

Jenny: I observed the shift first in the field of pedology. And that happened when the French, the Dutch, and the Belgians had to give up their colonies. Then a lot of science people returned from the tropics to Belgium, Holland, and France. They were highly qualified people; they had to be given jobs and they did excellent research. That situation was a stimulus to think and write.

Another trend is, of course, that peace has been going on, and after ten years, younger people come who in many ways have a better education, and great activity is going on in Europe. I would also say, in some ways, today, Europeans put more emphasis on theoretical studies than we do. We don't have a problem in Berkeley, but in other universities that may be a problem.

Maher: What happened to Grossenbacher?

Jenny: He found teaching jobs here and there. For a while he taught a course, substituting, at Mills College. He lives near here in El Cerrito, and he's very active in Gray Panther politics. He's running for office of some kind.

Maher: Oh, I know him. He's a short man.

Jenny: Yes. With a beard.

Maher: Yes, a goatee. Yes.

Jenny: Yes.

Maher: Do you know whether he had ever manuscripted beginnings of the papers that were never published?

Jenny: Well, he wrote one manuscript or two, that were not accepted.

Maher: Did you have any clue as to why they were not accepted?

Jenny: Maybe he had a problem with English, but I really don't know. He tried to hook up the mucigel idea and its chemical bonds with clay to explain soil structure. And he had some interesting ideas. I know Professor Babcock read one of his manuscripts and told him to restate the issues, but it never came about.

Maher: If you hadn't retired, if the projects had been continued, in what directions would you have wanted to go? Or what new techniques would you have wanted to see applied?

Jenny: Well, at that time, I probably would have done more of the same work to get better pictures, because when I showed the pictures to Professor Frey-Wyssling in Zurich, he remarked that they were not of high technical quality. But even with the low quality what we had found was significant.

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Jenny: At the humus institute of Professor [W.] Flaig in Germany, they tried our method and concluded there was too much distortion involved. But I knew the fellow there who did the work, and I talked to him and felt he wasn't as patient as Grossenbacher.

Finally, Grossenbacher developed the method of dropping the root-soil sample into liquid nitrogen and that would freeze it solid quickly. The problem with freezing was that if you freeze the sample slowly, you get big ice crystals which ruin the tissue. Dropping the specimen into a quick freeze medium creates small ice crystals, and thus preserves the structures.

Maher: At the time when Grossenbacher discovered that he could drop the sample into liquid nitrogen and freeze it quickly, was there any pioneering work going on with what was to become the freeze-dry method?

Jenny: Yes, that had been done. The freeze-dry method was not his invention, but he conceived the modified method to do our samples that way.

Combining Teaching and Research

Maher: We're going to be going back again to the period of the thirties, the late thirties--in fact, we'll probably go back further than that. But one thing that I've been quite curious about is all of the different projects that you were involved in in those first few years upon your arrival at U.C. Berkeley. There was tremendous variety in your work, and I am curious as to how you were able to handle all those different interests? Could you give some explanation of that?

Jenny: Well, the biggest job I felt I had, the biggest task was teaching--to get that lecture material ready and digest the many colloid chemical theories, and prepare myself because I was very critical. I had two courses in one morning, colloid science and soil genesis. I lectured from nine to eleven, so I was at the office at seven o'clock in the morning to review the material I was going to present.

Maher: Was that every day?

Jenny: No, that was Monday, Wednesday, and Friday. These three mornings were occupied solidly with teaching, because afterwards the students would talk to me and routine things had to be done. In the afternoon I worked until five o'clock, and usually I conferred with the graduate students. There was hardly a day I didn't talk to the graduate students or with Dr. Roy Overstreet or whoever was a post-doctorate collaborator. So this was a continuous stimulation of both sides and kept me going at a high pace; there wasn't an unfilled moment.

What did happen, however, I felt I was losing out on what might be called cultural things. I wouldn't go to as many evening lectures. I wouldn't go anymore to the symphony concerts or to the plays. So that I became confined. Also I didn't do as much gardening as I used to do because I was focusing strongly on the university activities.

But, I would say, because of that constant stimulation on the campus you are in a receptive frame of mind and ideas come and go, and it makes it easy to accomplish things.

I had difficulty with writing. I always wrote out everything several times for myself until I was satisfied. And, of course, I had good co-workers. This is a very important thing. I could trust Roy Overstreet with laboratory manipulations, and I could rely on the people in the greenhouse with what they were doing. So it was a good combination of things.

Jenny: I did a lot of studying. When I came here, the first thing I did was work through G.N. Lewis and Merle Randall's book Thermodynamics, a Berkeley classic. Incidentally, I did that with my friend Max Kleiber, who had been with me in Zurich, in Wiegner's agricultural chemistry lab, but now he was a professor of animal husbandry at Davis. We decided to go through the book because here everybody was talking about it. He and I met twice a week, either at Berkeley or Davis.

Then later, I had to do quite a bit of studying mathematics of diffusion, as I knew nothing about it. Graduate student E. Moreno was ahead of me. So I had to work on that. I must have done that very early in the morning or at night, or during office hours. Also, I spent practically every noon hour at the swimming pool, racing and taking a sunbath and having lunch there. Later on, I played badminton at the gym three or four times a week and then I usually went to lunch someplace--maybe at the Faculty Club. With Overstreet I played games of horseshoe behind Hilgard Hall.

It's astonishing to me what I accomplished then. Now I'm much more of a perfectionist, and much more on the defensive, whereas at that time I was, so to say, with the attack infantry, advancing, not thinking what might happen afterwards. Everybody I knew was that way. My friend, Zev Hassid, in plant nutrition, was that way. Everybody was running around.

For a while I had a visitor from an eastern university, I don't want to say which one, and he said on leaving that, to him, the most surprising thing was "that in Berkeley all of the professors are interested in what they are doing."

Maher: How would you compare your relationship with your graduate students as a lab team, as a research team? How would you compare that experience with what you had had under Wiegner?

Jenny: It was quite similar, except that I had many more contacts with the graduate students. Wiegner had a lot of contact with me, because I was his senior assistant, and his right hand. That's a different thing. In Zurich in the 1920s a graduate student would be given a thesis topic and then the professor wouldn't see him for months and months and months, till he showed up with a lot of thesis material or even with a finished thesis. Whereas, here, I was around the lab every day, to discuss things and encourage a student because I knew that later on I had to sign his thesis. Besides, I was interested for my own reasons.

Students came to me and asked me, "What should I work on?" Well, I usually submitted a series of topics and said, "You select what you like." But I was hesitant to let a student do research in an area I knew nothing about. Maybe that would have been a good thing for my own education, but I felt I wouldn't have been able to

Jenny: be of sufficient assistance to the student, because he had to face the critical thesis committee, that would want to know why he did and did not do such and such things.

Maher: To what extent do you think this involvement in the work of graduate students by you was influenced by Hoagland's pattern, as you outlined, of going around every morning and looking at the projects?

Jenny: I don't think I had a pattern outlined. It was a thing of mutual interest. I once had a student who went into an area where I was not really very competent, that was Reggie Laird. In helping with his thesis, I was not as successful as I would have liked to be, and one reason was that at that time I didn't know enough about diffusion. In a way, that was the case with Moreno, too, but not as marked. But after I had studied diffusion, and when [J.] de Lopez-Gonzales came from Spain--he was a professor of physical chemistry there--collaboration was perfect.

The Evolution of Jenny's Interest in Good Soils

Maher: Let's go into the background of your experience. In your write up here, you pointed out that a lot of it had gone all the way back to your experience in Switzerland.

Jenny: In Meiringen, in the Bernese Oberland, grandfather's large house of woodcarving activity was surrounded by dairy farms that kept me, a nine to twelve year old, occupied with cherished chores of animal care. My professional interest in agriculture was formed during World War I. Owing to food shortages each of us had a little cloth bag where we put in our small bread ration, to avoid fighting among family members.

I was active in the Boy Scouts and on many of our trips we visited isolated valleys where farmers would sell us bread, extra food and give us food stamps. One food stamp provided you with an allotment of rice. Somehow the Swiss government was able to get rice; I don't know how. But farmers didn't like rice, and they would give us their food stamps. That was an asset. Further we could buy butter from them, which was a big event. So this gave us an emotional background about growing food.

After graduation from high school and absolvement of a required year of practical farming, I entered the college of agriculture at the Federal Technical University in Zurich and became acquainted with research. The study of soil fertility was in the foreground. Journals and books extolled it. Soil fertility was the foundation of producing food. Pedology, the study of soil as a natural body, was of minor interest in Western Europe.

Maher: What were the ideas about soil fertility at that time?

Jenny: The ideas went back, essentially, to J. von Liebig, who published his world-famous book on chemistry in agriculture in the 1840s. The book initiated the field of study called agricultural chemistry. The emphasis was on mineral nutrients for plant growth, and it acquired prominence with the development of the fertilizer industry. A substantial number of chemical solutions for extracting nutrient elements from the soil competed for approval by researchers; the goal was to imitate the chemical action of plant roots. The methodologies changed with progress in theoretical chemistry, as they still do today. The biological aspect of soils were, I would say, underdeveloped. However, in a biological test for measuring available soil phosphorus the mycelium of a soil mold, Aspergillus niger was harvested and analyzed. So that was a microbiological soil test. I've forgotten whether it was developed in the United States or in Europe.

Pot testing with plants was very common. Continental experiment stations maintained fancy greenhouses with elaborate pot-testing equipment. The pots were put on wagons on tracks so when there was sunshine you could move the pots out of the greenhouse in the open, and in case of rain you rolled them back in. Pot testing, in various ways, was considered superior to the chemical soil analysis because the plant responds to all soil variables, but the pot test is time consuming.

When I went to Missouri in 1927, I started working with Professor William A. Albrecht, and we grew soybean plants on clay-sand mixtures, though not in standard pots. With E.W. Cowan we found that plants growing on Ca-clay acidified the clay in proportion to Ca-uptake. That had meaning also for my interest in the genesis of soils.

In Central Europe, I'm not sure about other places, the fertilizer companies would not send salesmen to the farmers to sell their kind of fertilizer. It was the experiment stations that decided what fertilizer a farmer needs to buy. So the salesmen merely got the orders, whereas here the salesman tells the farmer what he needs for his soils and tries to sell his products. That's why all this European emphasis on pot tests and chemical tests, so that the government people could arrive at reasonable estimates about what was needed.

Maher: So the view, according to what you're saying, was rooted in the work that had been done by Liebig?

Jenny: Liebig? Yes, Liebig, and others.

Maher: I'm having trouble pronouncing his name. It seems as though the emphasis in the soil fertility view was actually a view of what the fertility in the soil would be upon fertilizer being added, as opposed to the mode by which the soil, as a natural body, produced the fertility for plants.

Jenny: Well, you see, nowadays we know much more, especially in microbiology. For example, the process of nitrification was not comprehended until the late nineteenth century--or 1900. That the fixation of nitrogen from the air by legumes involved bacteria was not known until the 1880s. It was not realized that the microorganisms might compete with plants for fertilizer nutrients. The biological aspects were coming in very slowly. The emphasis was on inorganic chemistry.

When I came to Berkeley and was introduced to Albert Ulrich, who was planning to do pot testing, I became involved, and talked to him and talked to Hoagland. But I don't know how Hoagland got the money to build us a special greenhouse on the Oxford tract.

The pot testing in Germany culminated in [E.A.] Mitscherlich's work on the law of soil fertility or laws of plant growth. Way into the fifties we had in the United States lengthy discussions: "How significant is Mitscherlich's law? How valuable is it?" I was involved in these questions, too. With my world-wide pedological interests, I concluded long ago that Mitscherlich's law could not have general validity. Hoagland was totally opposed to it.

Maher: Could you explain that? Did he publish his opposition?

Jenny: I don't think he did. From his work with micronutrients and nutrient solutions he knew that plant nutrition is a very complex process. By using but a few crops, like grain crops and grasses, as they did in Europe, there was a restricted range of similar species. Hoagland knew from his experience with tomatoes and trees that the law couldn't work, and many other people thought so, too. Still, the great controversy continued.

Looking at Mitscherlich's law mathematically, I found that it was akin to a first-order reaction, like the law of simple, chemical mass action; it was merely formulated differently. I saw no reason why a complicated organism like a plant would want to follow such a simple law. In fact, of hundreds, nay, thousands of pot tests only about 50 percent fitted the law. New versions of the yield-fertilizer response curve are still being formulated.

Maher: When you got to the United States, what was your reaction to encountering the views of, let us say, [M.] Whitney or [C.G.] Hopkins, and the controversy between them? Essentially, I'm referring to the controversy about whether the fertility of American soils was an exhaustible resource or whether it was inexhaustible?

Jenny: Well, during my year in New Jersey I heard nothing about these ideas. I became acquainted with them in the Middle West, not by hearing about them but by reading old accounts.

In many ways our colleges of agriculture and experiment stations seem provincial. People in New Jersey know very little about the research that is going on in California, and vice versa, except for the few individuals who are privileged to attend national meetings. During my years in the East I met nobody who had heard of California's great E.W. Hilgard. North America is a huge continent with many different environments, crops, agricultural systems and interests.

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Maher: Okay. Now we can wade through that more thoroughly another time. It would probably be best if we go right on into the pot test.

Jenny: Well, but I shouldn't be spending too much time on it now.

Appraisal of Fertility of California Soils, and Its Agricultural Consequences

Maher: Okay. [brief tape interruption]

After 1940, evidently, you parted with Ulrich, as far as working on the project, but then as the war years continued and the victory gardens, you got led back into pot testing. Can you explain the place of the victory gardens and how it led you to the pot testing again?

Jenny: Albert Ulrich and I drifted apart because he became interested in leaf analysis for monitoring nutrient deficient soils, which he considered more promising than pot testing, and I became engulfed in lab research of the reductionist kind.

As the war started the victory garden movement became prominent, and various faculty members gave talks to Berkeley groups. It was a popular thing to do. Entomologists would address the meetings about insect pests and plant diseases, and soils people would tell the audience about soils, but there wasn't much we could say about fertility of Berkeley soils. People didn't care hearing about soils in general; they wanted to know what to apply to make the heavy soils more fertile and tractable.

At that time [M.M.] Elgabaly from Egypt was with me as a graduate student and he was interested too, and we decided to conduct pot tests with six-inch flower pots, and various plant species. We found

Jenny: that Romaine lettuce was sensitive to nutrient deficiencies. So we started a systematic survey of Berkeley's empty lots, because the plan was to convert them to victory gardens. We did not realize that the military also had plans for these lots. They put barracks on many of them. Articles appeared in the local newspaper about what soil testing we did and what more should be done.

I thought this would be just a brief intermezzo until the war was over, but then [R.E.] Storie and his group of soil surveyors became interested in our activity, because in the Storie index rating of soils he had nothing substantial about fertility of California soils. He could see promise in our test. The way it worked, you see, it didn't take more than a few weeks to get results. We germinated lettuce plants for two weeks and transplanted them to pots, and in four more weeks we had a good answer. Thus we began systematically testing the soils of California.

When the Soil Conservation Service heard about it, [L.] Wohletz became interested and we collaborated. He furnished a greenhouse helper, first, George Schaefer and then Ralph Nelson. The soil samples arrived in hundred-pound lots and we had to dry them and sieve them and lay out the fertilizers. This was a thing for concern. Fortunately, Ulrich had developed a method to check if a worker made a mistake in weighing the fertilizer. Ulrich would give the workers a weighed amount of fertilizer, but wouldn't tell him exactly how much. After the worker had removed the required number of aliquotes, Ulrich would weigh the remaining amount of fertilizer and could tell whether or not a mistake had occurred.

Because the lettuce plants grew in a relatively small amount of soil they responded quickly to treatment, but out in the fields where soil may be deep a crop response to nutrients might be marginal. Hence we had to calibrate the lettuce yields in pots with field tests of various crops. It was here where the Soil Conservation Service became important. They set up many experimental plots. Also the University of California Agricultural Extension Service had done field tests, and in that way we had available over 100 field experiments for lettuce calibration. We collected soils from the check plots and subjected them to greenhouse studies. The entire scheme proved eminently successful and provided an instructive view of the nutrient status of many hundreds of California soils.

In two areas we ran into trouble. The chairman of the Department of Truck Crops at Davis--which includes lettuce production--came to Berkeley and told me categorically that I should <u>not</u> attempt to inform the lettuce growers of what we found. Actually, I hadn't planned to do that, because the tests were conducted to assess fertility of soils, not for us to advise the lettuce growers. The truck crops department wanted to give the growers new information,

Jenny: and our fertility aspects offered a big opening to strengthen their rapport with the growers. The truck crops people feared intrusion by another department.

Another area of trouble was Wohletz's habit of coming into the greenhouse, lining up pots and plants, and taking photographs. He would incorporate soil information, reproduce the display and distribute neatly mounted copies among the farmers of the Soil Conservation Districts. The agricultural extension people didn't like that at all, and they put pressure on the dean of agriculture to stop this whole project.

Well, I was on good terms with Dean Claude Hutchison, because, after all, he had facilitated my coming to California, and had made me a department head. On occasion I served him as speech writer. A compromise was made, and Dr. [W.E.] Martin was hired by the Agricultural Extension Service to participate in our enterprise and advance studies of pot-testing problems and field-experimental problems among the farm advisors.

Actually, the issue was more fundamental. The farm advisors had long complained they didn't get enough support from the researchers. The farm advisors wanted the faculty to work on problems they were facing in the field. And the faculty wanted to work on problems they considered important. So there was always a schism and now it showed up spectacularly. So they pushed the appointment of an extension specialist in soil fertility, and that was Martin who came from Arizona.

Maher: And was he considered a compromise by you because he brought in the expertise in soil fertility?

Jenny: Well, he didn't have the expertise. He acquired it on the job and he was very good at it. He secured input from the farm advisors and from the extension service, thereby balancing the input from the competing Soil Conservation Service. After a while the Soil Conservation Service discontinued collaboration, allegedly having been under pressure from higher-ups. By that time I had hired [J.] Vlamis to supervise the greenhouse work and take over the pot-testing service. Eventually, at Berkeley over 1000 soils were processed.

In 1950 we published in Hilgardia a condensed version of our statewide activities. The article appeared at the right time and it was to play a role. An immediate response came from the fertilizer industry. I had had little contact with it. "The College of Agriculture is not interested in fertilization; it recommends but nitrogen, and small dosages at that," the industry lamented, "but now Hans Jenny is giving us a chance." The industry invited me to conferences and conventions and asked me to lecture about the nutrient deficiencies of California soils.

Jenny: Also, the California Farm Bureau became interested. I remember I was to give a talk at eight o'clock in the evening, a nature talk, but the afternoon session kept dragging on and on and I wasn't called upon to give my talk until at one o'clock in the morning. [interviewer laughs] And I was told that everyone would be happy if I'd be brief. I announced my title and quit immediately, under applause. Everybody was tired and half asleep.

I attended quite a few California Farm Bureau meetings. The farmers come and express their feelings and often it's their only opportunity to talk at a public forum, so it tends to be endless. In contrast, at scientific meetings the chairman will stop a speaker after the allotted fifteen minutes, but you can't do that at farmers' meetings; you have to go on.

The American Farm Bureau had a consultant for the whole United States named [G.] Scarseth, from Indiana. Prior to his job, when I was still at Missouri, he wanted to come there and work with me on declining soil fertility in the Middle West. But then he went to the tropics, and I came to California. After he came back from the tropics he became the farm bureau's agronomic advisor. When told about my lettuce-soil testing here, he put two and two together: how soil fertility declines if nutrients are not replenished, and that in California the older soils and the soils in the hills need already nutrients. He urged the California Farm Bureau to establish a fertilizer cooperative to get out of the expensive dependency on the fertilizer industry, which they did.

Maher: To get out of the what?

Jenny: Out of the claws, so to speak, of the fertilizer industry. They did the same thing with insurance companies. They didn't want to pay for these fancy insurance company buildings that were going up every place, so they set up a cooperative insurance policy among the farmers.

As the California Farm Bureau readied itself to support a heightened role of plant nutrients, the fertilizer manufacturers intensified production. The brand-new Chevron plant in nearby Richmond had based its expectations on our pot culture report--which appalled me--and rumors had it that fifty million dollars had been invested, a big sum in those days. Apparently it was successful because Chevron expanded construction and built additional plants in the eastern United States.

Maher: Subsequent to that?

Jenny: Subsequent to that.

Maher: So, nationally, you feel that the birth was here in California?

Jenny: No, I wouldn't say that. More fertilization was practiced in the eastern United States than in California, much more. Hence I would not want to say that I had helped push the fertilizer consumption in the eastern United States, except for my ill-fated proposal to fertilize soils with ammonia gas, for which [F.H.] Leavitt obtained a patent in 1942.

The rise in use of fertilizer in California had been slow, because the money at that time was in orchards and vineyards, and the orchards were located on the best and deepest soils. So their fertility needs were not great, and the truck crop industries had not yet developed to the extent that they did later.

Maher: That is, to the extent of depleting topsoil?

Jenny: No, of being significant. Because the shallow rooted, fast-growing truck crop plants need more fertilizer than the roots of a tree that explore a large volume of soil and grow throughout the year.

Maher: What was the consulting that you gave to Standard Oil concerning this?

Jenny: When they built that Chevron plant in Richmond, California, I was involved. And throughout the years I was in contact with them in some advisory capacity or otherwise. For the oil companies it was a natural thing to go into the fertilizer business because they produced a lot of hydrogen gas that they could convert to ammonia and ammonium fertilizers, including supplemental phosphate fertilizers.

Maher: Why would it be natural for the oil industry to get involved in the phosphate production?

Jenny: Just as a supplement. As we said, the ideal fertilizer for many of the California soils appears to be ammonium phosphate. If manufacturers take on fertilizer distribution they need to carry also the other types of fertilizers because the farmer doesn't want to go to two different companies to get what he wants.

Eugene Hilgard's Broad Conceptual Approach to Soils

[Interview 11: June 25, 1980]##

Maher: Dr. Jenny, you have often mentioned that in Europe you were aware of Hilgard, an important figure in soil science, and you were surprised that he seemed to be underplayed within the United States.

Maher: Could you explain your reaction in that regard when you got to Berkeley, specifically in respect to the people that you found there and the ideas that people had of soil science?

Jenny: I heard of Hilgard first,I think, in Wiegner's lectures where he discussed the Russian scheme and the comparison of arid and humid regions. He quoted Hilgard's analyses. Then, when I was working on the soils in the Swiss Alps, I became interested in changes in organic matter, and so I started speculating on the climatic affect on soil organic matter. I tried to formulate some sort of a model. It was, in retrospect, very crude and wrong. Still, it was a model aimed at getting information on the organic matter content of soil in relation to climate.

I needed data from the arid region. There was still the idea that desert soils would be high in organic matter because, although the vegetation was sparse, there was no decomposition. So I had to have figures. I looked in Hilgard's book which came out, I believe, in 1907 and got hold of the data from the arid region. I got interested in Hilgard and read more in his book.

Then, when I came to New Jersey, Waksman, of course, knew of Hilgard because Waksman had studied at Berkeley. But when I came to Missouri, there was total silence when I asked about Hilgard. They had never heard of him. I had many contacts with people in Illinois, Wisconsin, and Indiana--the states surrounding Missouri--I was curious and I always asked about Hilgard, and they didn't know the name.

When I got to Riverside for that sabbatical year, of course, Hilgard was in the picture, very much so.

Maher: How so?

Jenny: Because Hilgard did a lot of work on alkali soils, of arid regions, and Riverside is in that area. We had alkali soils in the Imperial Valley, so Hilgard's work was known. Of course, Kelley was a student of Hilgard. He got his Ph.D. under him. When I came to Berkeley, Hilgard was very much in the picture. In plant nutrition, Hoagland and especially Burd, knew very well about Hilgard because Burd took over some of the work that Hilgard had been doing. But in soil technology under Shaw Hilgard was barely mentioned or only in a negative way.

Maher: Could you be more specific about this "negative way"?

Jenny: The "negative way" was that Hilgard was not a soil surveyor. He constructed soil maps of the southern states, when he lived down in Mississippi, and soils were placed in broad groups. His emphasis was on parent material, on the rocks, vegetation, and chemical analysis of soils. He discussed horizons and processes, but not in

Jenny: the sense of the soil texture school of Whitney. Thus he was not considered to be in harmony with soil surveys and he had also criticized the Whitney school. Hilgard said they rushed over the landscapes too fast, their surveying was very surficial because they had to cover a lot of territory to produce all of these maps.

He wrote letters to the editors in which he said it would be better to cover smaller areas, and more accurately, and more in detail, and with follow-up analyses. In the early days there were no soil analyses in the reports. A little later, they started to run mechanical analyses, but at the beginning there was nothing. Moreover, Hilgard always wanted the soil survey to be included in the geologic survey because he had a high opinion of that organization, and he thought that's where it should be housed.

So his conflict with Whitney and his school did not endear him as a proponent or defender to the soil survey people.

Maher: In your own reaction to Hilgard's writing, did he attract you to any extent with respect to pushing forward ideas, including those on the theory of soil science?

Jenny: Yes, very definitely. His idea of arid and humid soils really put soil science on the map. During his time, the soil survey staff had not heard--or at least did not accept--the Russian ideas about the climatic principles of soil science. Their ideas were always related to regionalism; they had provinces, such as the Great Plains province, and the Northeastern province. That's understandable because soil surveying and soil science have, in many ways, emphasized this approach because the regions differ profoundly. Also, people who study soils in a region usually have very little access to other regions. The people in Missouri made soil maps and soil surveys of Missouri and did a very good job, but they had no opportunity to work outside the projects, except perhaps to go to a meeting now and then. They would never go to California. It was either too expensive or no opportunity was offered.

I remember when M.F. Miller taught his course, "Soils of the United States," which I took. Well, he ended with the Great Plains area and he said, "West of it there are no soils, just parent materials."

It also meant that they lacked contact with distant areas. They would not read the literature about them because in California, Arizona, Washington, and Oregon, a lot of the then-current literature dealt with irrigation; irrigated soils with alkali problems, or salinity. Those problems were totally absent in the eastern part of the United States, hence they were not considered. So I am not surprised that regionalism dominated and did not foster the broad principles of soil science.

Maher: In many of your writings, you have spoken about the tendency to have a split between the theoretical and the practical or between basic science and applied science and that often there is too much emphasis on applied science. How did you assess Hilgard in that panoply of theoretic to practical? How did you view the people that you were meeting in the United States in this respect? [pause] Did Hilgard seem to be pushing soil science forward onto a theoretical plane?

Jenny: Of course, I never met Hilgard.

Maher: But through his writings?

Jenny: But through his writings and his letters--I read some 20,000 of his letters and I copied many. So I think I have a good idea of Hilgard as a person. I would say he did not stress the difference between theoretical and applied soil science. He found both areas very interesting and consuming, as I do. I wouldn't say that at that time he followed theoretical soil science. I would rather use the word conceptual. He had the idea of static soil forming factors, but he did not formulate them into a broad scheme.

He came from Mississippi, where it was hot and humid, to Berkeley, where it was arid. He continued his soil analyses and he found enormous differences right away. Since he had profound geological training, he clearly separated the soils according to different parent materials, and he did not fall into the trap of saying soils are high in calcium in California because calcium is inherited from the rock. He was very clear about this and he was also a good botanist. So he combined these factors, but he did not assemble them into a conceptual scheme.

I think he was the first to really bring in the climatic principle as far as mineral substances are concerned. True enough, the Russians had, during his California time, proposed a climatic principle of soil organic matter. But he was the one who showed it applied to reactions, to calcium content, to kinds of minerals, and to the whole inorganic field of soil formation. What he did is really more striking because people will readily accept the effect of climate on organic matter, but to demonstrate it for the soil weathering processes and the mineral matrix of the soil was unusual.

Maher: Considering the rejection of Hilgard by Shaw, and evidently from your writing here, by Dean [Thomas] Hunt, who you say thought little of Hilgard, do you think there might have been an element in that rejection of just not appreciating the necessity for conceptual development that Hilgard was making or broaching, or was it just this conflict between Whitney and Hilgard?

Jenny: It was also in terms of the purpose. Now, Dean Hunt, looking over his legacy, wrote several bulletins on giving advice to the settler. This was still the time when people came West from the East and

Jenny: wanted to settle. The soil survey was visualized as the great aid, leading to making soil maps on a large scale so the farmer, when he bought land, could see what he had. Whereas the soil maps of Hilgard were more conceptualized and on a smaller scale, and you could not tell for sure what soil existed on your piece of land except in a broad way.

So Shaw symbolized this new survey method in Berkeley and he and Dean Hunt were in close association in pushing this information. Certainly, to the settler, what Hilgard had mapped didn't mean much. The work he did, say, in the valley on alkali and salts, like at Tulare, finally was stopped because the people of the town of Tulare complained that their county was associated with a bad thing. So the real estate people fought Hilgard and finally stopped the experiment station in Tulare.

There is this difference in objectives. Also, I would say that Shaw probably did not have the background in chemistry and physics that the people in plant nutrition had. They were really the spiritual descendants of Hilgard, so to speak, and they operated in a different intellectual climate and sphere.

Writing Hilgard's Biography

Maher: We'll come back to when you mentioned Hoagland and his company as descendants of Hilgard. But I need to get an idea of where you, upon coming to Berkeley, were getting additional information about Hilgard? When did you start developing an interest in examining his letters and finding out what had happened? How were you receiving primary information about Hilgard?

Jenny: I received letters and phone calls from a history professor down at UCLA. He had begun to write a book on Hilgard.

Maher: When?

Jenny: That was while I was here. That means it was in the late 1940s. He was totally at a loss how to comprehend the soils work Hilgard did. He had no knowledge of soil science or soil chemistry and soil physics--nothing. But he had made contact with Hilgard's daughter, living in the old Hilgard house on Bancroft. So I went to see his daughter to get information this fellow from UCLA had asked me to get and so I got acquainted with her. When she found out that I was from Europe and could speak languages that Hilgard spoke and wrote in (he wrote in five languages), she was rather keen that I take this up. I did not do anything until a person from the East, who wrote history books on botany, wanted to write about Hilgard.

Maher: This is all in the late forties?

Jenny: Yes, that came now in addition to the UCLA historian. So I had to make a decision. Should I be involved or not, and I talked to Dean Hutchison: what should we do now? Miss Hilgard had shown me all of the material she had, and the question was, should this go East or to Los Angeles, or should we make an attempt to do something here? Hutchison encouraged me to write a biography of Hilgard and that's what I did. So I got to know a lot about him.

Further Reactions to the Work of Hilgard and Jenny

Maher: In the early period of your being in Berkeley in the mid-thirties, you had contact with Kelley who was a student of Hilgard.

Jenny: Kelley told me a lot about Hilgard, yes, and of course Hilgard was a very famous man, and as it happens in all these cases, if you are put on a high pedestal, people try to pull you down so they get a chance to be somewhere up there, too. Kelley was derogative of Hilgard because Hilgard had missed the ion exchange principle and, therefore, had made a number of errors in alkali reclamation. Of course, Kelley deserves the credit for having continued that work and improving on it. Kelley was quite self-serving but in spite of that he did a lot of valued alkali research throughout the state.

Maher: Earlier you had characterized Kelley as being criticized down south at an experimental station for being too theoretical. His clay work, they felt, was theoretical and unappreciated.

Jenny: Yes.

Maher: Did he appreciate Hilgard as a man of concepts?

Jenny: Yes, I would say he had basically a high opinion of Hilgard.

Maher: In respect to generating concepts?

Jenny: Not so much in generating concepts, but of his pioneer work on alkali, and his ideas on weathering, and how alkali was formed, his whole interest in these problems. Before I got to Riverside, Kelley had done a lot of very successful work on alkali reclamation in the Fresno area. He told me that at that time this work was very prominent and he was talking over the radio and giving lectures all over the state about these successes. So that was great for the Citrus Experiment Station, but later on when he got into clay studies and ion exchange studies--and especially through me, when he

Jenny: became very enthusiastic about that--the people at Riverside, especially extension people, felt he was not delivering the information they needed to advise the farmers.

Maher: Now, coming up to Berkeley then, what kind of impression did Hoagland give of his having been in contact with Hilgard?

Jenny: [pause] I can't cite specific instances. It was more or less an overall emotional response, you might say. It was not based on specific instances or specific features Hilgard had produced, but an overall emotional appreciation of what he did because, after all, his soil work ended say about 1890 and afterwards the burden of deanship in the College of Agriculture prevented him from keeping up with modern science. But at least he pointed out the important problems.

Maher: When you speak of Hoagland and his company being trained well in chemistry and physics and basics, being the group that was carrying Hilgard, was that your impression at the time when you arrived here at Berkeley in the mid-thirties or was that an impression that you developed in the late forties when you were looking at his background and going backwards?

Jenny: I can only say that in Shaw's group, Hilgard was not mentioned or only negatively, whereas in plant nutrition there was a positive reaction. Hilgard missed many things and many things were unknown. But Burd's work on soil solutions was related to Hilgard's work. Burd taught agricultural chemistry, and on campus there was a division of fertilizer applications and Burd had access to that. Burd had been in Washington with the Wiley group, and with Cameron, where he got to know the soil solution theory and that fitted in with Hilgard's work. Hilgard always measured soil solutions. He did not measure exchangeable ions. He did not know about that. He analysed water extracts, or soil solutions, or hydrochloric acid extracts. The method we use now--displacements with ammonium acetate and so forth--were not known to him.

Maher: You were very much an idea man when you came to Berkeley. When you had been in Missouri you always wanted to talk about ideas. You've mentioned that when you were here in Berkeley you talked a lot with Overstreet. You bounced ideas back and forth a lot. How did you feel as a person who liked to develop ideas in soil science amongst the general people that you were finding in the department, the people you came into contact with in the first five or ten years leading up to Shaw's death, let us say?

Jenny: Hoagland was always interested in ideas, although he was not a producer himself. Then there were all the young fellows like Hassid, and Barker, Dore. They were all eager to discuss things. I won't say, for example, in soil technology, that Shaw didn't have any ideas. He had a very profound idea which at first I did not

Jenny: recognize until I wrote Factors of Soil Formation, and that was what we call now the chronosequence. He talked about the San Joaquin family and that was a chronosequence which still exists. So there was an integration of soil survey which I evaluate very positively as a conceptual thing. So I would say in that area, on that point, he was way ahead of many others.

Maher: When you had published your nitrogen curves back in Missouri, some people felt that you were too theoretical, that you were kind of getting lost in mathematics. Isn't that correct?

Jenny: Well, some; we had a number of reactions. But I remember now since you talk about it, it comes to my mind, the head of soils, M.F. Miller at Missouri, he didn't like very much the idea that I was--

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Maher: --that you were extrapolating the curves outside of the state of Missouri.

Jenny: Yes, I was going North, I was going into Iowa, and I was going into Nebraska and Kansas to get samples. He didn't like this idea because it got him into conflict with the people in Washington. The federal people felt that they were in charge of the overall soil research on a national scale and that the states--university and state experiment stations--were to restrict themselves to the soils of the state, and there I came and made these curves clear across everything. So that was in a way a threat to the people in Washington.

Maher: Going back to the occasional feel that your curves were too theoretical or too mathematical, did you ever get a reaction like that amongst the people at Berkeley?

Jenny: No. Oh, no, I got full support at Berkeley for almost anything I said. I was really sitting in a splendid environment both physical and intellectual.

Maher: You also mentioned back in the Missouri period that though people would encourage your ideas, you didn't really feel that you were having a dialogue with people. Did that change when you were here? Let's exclude Overstreet because obviously that was--

Jenny: No, I would say after Shaw died I did not have the dialogue with the people in that former department. Shaw was interested or would listen and discuss the broader aspects. But the group he had did not--say Weir and Storie. They came through different avenues and had other attributes which were valuable and strong. But they were not in the field of what you might say theoretical science.

X WRITING FACTORS OF SOIL FORMATION

Some Early Background

Maher: Why don't you just rather freely gather together for us how the different strands of development toward your ultimately teaching pedology and publishing the book come together. Can you gather the strands for us? [pause] It was a very theoretical--

Jenny: It was. When I was in plant nutrition, my pedological activities were, minimal because I was immersed in soil chemistry and plant nutrition ideas. But I talked about it, and even in the colloid chemistry lectures, I may have referred to it.

So the great change came when suddenly Professor Shaw died. Professor Shaw was very friendly toward me and encouraging. I don't know his background but I heard he changed his name during World War I. He was from Pennsylvania, so I assume he had a German name. So Shaw was his second name. I don't know whether that entered at all, but I would say he was very positive. But he died suddenly, fairly young, of a heart attack.

At that time, Bodman, who was in Shaw's soil technology department and who was a man of ideas in soil physics and also worked in soil genesis, and the people in plant nutrition, and probably Carl Sauer, in geography, who was interested in soils--they all felt that a change should be made, a reorganization of that department. That was aided by the transfer of Kelley from Riverside to Berkeley, and he was made head of that new department. Overstreet and I agreed to be transferred to the new Department of Soils, and it was decided that I was to give Shaw's course on--I've forgotten now the exact name--the pedology course which he taught.

Storie had assumed that he would be teaching that course and at the beginning I am sure he was very negative. So, I had this new position. I had a new title and people knew me in the East. Now,

Jenny: I was really made visible--sort of put into a window--to show "that this is the California exhibit" in terms of ideas and so I felt often on uncertain grounds with my graphs. I knew the curves were correct observationally, but how did they hang together? What were the underlying causes? I wasn't sure. I felt insecure.

As soon as I was teaching the course I began analyzing the concepts and felt I should devise a soil system that is broader than the soil provinces and that would look beyond California or the state of Missouri, involve broader principles of soil science, overcoming so to speak the regionalism.

Also, the way I selected sample sites in the field was completely different from what the soil surveyors were doing. I would outline where I would go for sampling; I had a scheme, a plan, where and how to sample soils to answer certain questions.

Maher: Why didn't you encounter trouble in respect to going beyond California in the manner that you had in Missouri since this was a state school?

Jenny: My sponsors were the people in plant nutrition, Professor Kelley, and others on the campus; imposing restrictions never occurred to them. Moreover, their relations to the people in Washington were not as close as those of the people in Missouri, or in Illinois, or Indiana where collaboration was almost a marriage.

Maher: Why not?

Jenny: I think it has to do with the state of California which feels itself almost like a kingdom in a way, a unity, an enormous thing, whereas in the midwestern states you don't have that. You are more part of everybody else because you can go North, East, and West and you find the same thing, whereas here are natural boundaries.

Maher: So California academically wasn't as tightly engaged in the federal network?

Jenny: That's right. At least in soils, we never had federal people on our staff--no combined appointments.

Maher: Was there resentment of that by the federal groups?

Jenny: Yes, I'm sure, because Shaw had independent ideas--Shaw's concept of the soil family, Storie's classifications of California soils, and his Storie index. These were not welcomed in Washington, and so there was no communication favorable to close collaboration. I know Storie often complained that the Washington people were making all sorts of decisions about soil classification and soil definitions

Jenny: without consulting experiment stations, or him. But I learned later that they did consult experiment stations, but mainly their buddies in the Middle West and not the people here.

Maher: At any rate, you felt rather unfettered in terms of going out and getting samples.

Jenny: I had only encouragement. I never ran into any fences.

Maher: So please return and continue with what you were explaining about developing the course and unifying your ideas.

Jenny: While I was lecturing, and in a way notes were put speedily together, I had to have a scheme about the course, and learn many things I did not know. I did not know enough about the soils of California. Storie taught a course on soils of California. That was the trade-off. He was an authority on that subject. My theme was then the broader field. That was another encouragement to take the broader views. Probably I was still influenced by the Russian ideas of emphasizing climate, and by other factors, rather than the mapping.

Kellogg told me not too many years ago, that the Russians never learned to map soils the way Americans do, and I probably would agree with him, except I wouldn't accept the word learned. They had different perspectives and needs.

Maher: Could you explain that more?

Jenny: They thought more in terms of processes and factors than in local mapping. Of course, the latter goes back to Whitney who wanted maps to help the settlers. It was considered important, and the Congress in Washington thought so too and supported it. I taught the things I had done at Missouri and what other people had done. I talked about Hilgard's work and that of his successors, like [G.N.] Coffey, or other Hilgard counterparts in Washington. When I say Coffey, I refer to soil surveyor Coffey who extended Hilgard's pedogenic work and collected more analyses.

I brought in all that, and I brought in aspects of history of soils in the United States because I was thinking about what had happened to the people who came from Europe. I visualized: suppose all of the settlers had arrived by plane, flew over the United States and had parachute randomly down on the land. Some would be on good soil, some would be on poor soil, and we would have an economic stratification right from the beginning.

Jenny: It interested me how the pattern of soils on homesteads worked in the United States. I was intrigued by a book written by a historian at Yale University--I believe Hulbert was his name--on the role of soils in the history of the United States. I could see that role in Missouri, I was already interested in that. On field trips we went to the Ozarks and walked through scrubby woodland on poor soil and encountered an old cemetery with a lot of German names on the tombstones. It belonged to a vanished settlement. Religious people might have come from Germany to land that was available. They had no knowledge about soils in the United States, and they couldn't make a go of it. So, I was interested in that.

I became interested in prejudices about arid soils, say their lime horizon, which West Europeans don't have and we don't have it either in the eastern United States. Settlers who moved West were suspicious of soils which had calcium carbonate horizons. They called it white gravel soil and they avoided it.

As I told you once before, in Missouri the first settlers would not cultivate the grassland soils, which are the best soils. They said that a soil that cannot support the forest cannot be very good and thus they occupied the forests, cutting down the trees. The people who came afterwards and who had no forest left over moved onto the prairie and found that it was really super-fertile compared to the forests.

Many of these things I brought in. Some of it was anecdotal. When I drew curves on the blackboard and intelligent students would ask questions--you might say embarrassing questions I hadn't thought about--all these things forced me to set up a sort of philosophy of the course I was giving. I started digging at the definitions. First, I looked in books about the soil forming factors, what they are, and it was all very confusing.

Defining Parent Material

Maher: Could you be more specific about the confusion?

Jenny: Some say that the soil forming factors are the causes of the soil. That looked pretty good as far as climate is concerned. But with the time factor, I couldn't see how time can be a cause. At that time, I was interested in philosophy of science; I had been quite active in a philosophy seminar group in Missouri. So I wondered. I had, you might say, a background of asking questions,

Jenny: what these expressions really mean. I did a lot of reading on the causality principle, to learn how that could be applied: is it the overall principle that determines the distribution of soils, and why soils are what they are?

Maher: What did you read on causality?

Jenny: At Missouri I had read Campbell's book on the foundations of physics and chemistry.

Incidentally, around 1950 I heard about a symposium on philosophy of science, and that the Campbell book was mentioned as the outstanding book of the first fifty years of this century. The notice encouraged me because I had relied a lot on what Campbell had to say.

Let me see now, what sort of strange definitions were given by pedologists? Besides causation, the forces. Yes, a group talked about forces. Well, again, time cannot be a force. How can parent material be a force? I talked to Overstreet about it and I said, "I'm really getting confused about what I'm teaching." I said, "For example, what about the forces?" He said, "It doesn't make sense." By talking to others, I gradually realized what parent material should be, and that was based on data from a botanist in England. I think [E.J.] Salisbury is his name. He measured leaching of carbonates in sand dunes in England. He had dunes dated historically from old maps, and I plotted his data on curves. I remember, I had this curve on the leaching of calcium carbonate, how the dunes had less and less calcium carbonate as they became older and older. I could see plainly that the extension of the curve backwards must go to the original dune, to the original calcium carbonate content of that dune, and that quantity existed at the beginning of soil formation. So the term just occurred to me then, "time zero," and the composition of the soil at time zero, that must be the parent material.

So I got that under control. Overstreet brought up the question about temperature of the parent material, that a sand dune in northern Alaska would have a different temperature than a like sand dune at the equator and the two dunes would have different climates before they started developing soils and thus they would be different parent materials and that would interfere with the idea of soil climate functions of identical parent materials.

Jenny: I kept thinking about that and finally concluded on defining all parent materials for a chosen reference climate. Thus all parent materials would be defined at twenty-five degrees centigrade and having a given moisture content. The latter certainly was important for clays. With clay parent materials we have to specify the water content.

With the time function, I was asking: when does a parent material become soil? This question was discussed in the literature extensively and people tried to differentiate soil from parent material. The time curves of calcium carbonate leaching forced me to say that as soon as the parent material begins to weather it becomes soil. If that's not accepted I would have to set the boundary at some later time and it was not clear at what point that decision should be made because there was a continuous change. So the difference from parent material to soil had to be right at the beginning and that's why I found the time zero concept very fruitful. That led to the idea of an initial state in a reference frame. If we had sensitive instruments we would observe small changes in properties.

So I felt I clarified the definition of parent materials and introduced the notions of initial state and time zero. This was quite exciting and I discussed these with Overstreet. "I like it," he said when I told him how I defined the parent--

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Maher: So Overstreet liked that terminology since he was used to reference states?

Jenny: Yes, because that really was also the vocabulary in physical chemistry. I guess you get ideas--suggestions--from all sides and you can't always tell what is original and what is an influx.

Defining the Biotic Factor

Jenny: The factor of topography wasn't too much of a problem, but the biotic factor, that posed a real difficulty, and while people have accepted the parent material as the initial state, there is, I would say, controversy and hesitancy in seeing the biotic factor the way I see it.

Maher: Why don't you explain the development of your thinking along that line, the first terms that you used and how you bounced back and forth?

Jenny: It probably goes back to when I worked with Braun-Blanquet, the botanist in the Swiss Alps, and when we discussed how soil and vegetation evolve together. To what extent does soil affect vegetation, to what extent does vegetation affect the soil, and how can the two actions be separated? Later, fancy words appeared in the literature, like loops from the information theory, on how interaction was to be handled.

In the technical literature a soil formation equation by Professor Shaw has soil as a function of vegetation, and we have a formula by Tüxen, a German botanist, in which vegetation is a function of the soil. Hence we have two contrary formulations, switching cause and effect.

It took me quite a while to convince myself--and probably as a result of talking to people like Professor [Herbert] Mason, who used to teach ecology, to Overstreet and to others like Sauer--to come to the conclusion that vegetation was not the proper word to use for a state factor. The botanists made long ago the fundamental distinction between vegetation and flora of an area. Zoologists don't have this idea or don't have different words for these two things.

Maher: Where was the difference in your view?

Jenny: According to botanists, the flora of an area is a list of species, how many species and their names, whereas the vegetation refers to the amount of biomass produced--whether the corn plant is tall or small, or in poor shape, or in good shape--this would be vegetation. It would be what the botanists call the phenotype compared to the genotype, which refers to the genes of an organism. I don't recall how I made that transfer from vegetation to flora as being the biotic factor, and, like the parent material issue, it probably was a gradual thing.

With colleagues and students I discussed pot experiments where you put seeds in a flower pot containing soil and the seeds grow and depending on the soil you get big plants or little plants, which is the vegetation. The action of putting the seed in the system, that is really the crucial thing. The seed species is the flora-type.

Jenny: Looking back now, when I wrote Factors of Soil Formation, I wasn't a hundred percent sure, or I wasn't consistent in separating vegetation and flora. Certain headings of sections are, I would think now, misleading because I talk about vegetation. I have concluded, therefore, that the vegetation is a dependent variable and flora can be the independent variable. I identified the latter with the soil forming factor as the influx of species, the flora of influx.

Some years later, I discussed that thoroughly with other people like [R.L.] Crocker and another Australian, and we agreed that the core of this idea was satisfactory or plausible.

There are several ramifications regarding the "species rain," the influx of the species. For analogy, I call precipitation a state factor; the soil forming factor precipitation. But from the viewpoint of soil chemistry, that is not necessarily the proper moisture criterion. To a soil chemist, the water regimen inside the soil is the crucial thing. If there is a close correlation between that water in the soil and the precipitation, then the precipitation is a proper state factor to use. On slopes there may be runoff. From the viewpoint of erosion the runoff part of precipitation would have to be used. This is very clear in Arkley's leaching factor; for the leaching process the water which percolates the soil is the crucial thing. Both runoff water and percolation water are related to the rainfall, but not necessarily in a simple fashion.

Maher: Had you been thinking about this in the period when you were writing the book?

Jenny: The Factors of Soil Formation?

Maher: Yes.

Jenny: No, I bring that out now to explain some of the difficulties, and one such difficulty occurred in the biotic factor, and I argued back and forth with Crocker in the 1950s. All right, the influx of all species, but actually what counts is the species that are growing; that is what will affect the soil; not all what comes in, except, that you get carbon and nitrogen in seed influxes, and that sort of thing. Crocker restricted the biotic factor to the effective biota. He said the vegetation is the effective biotic factor. I say, it is the flora that is growing.

Crocker also thought vegetation was affecting the soil independently, but I don't see it that way. You put a seed in a pot and the seed grows, but how it affects the soil depends entirely on how the environment of the plant, including substrate, controls the behavior of the plant. To me the "effective biotic factor," meaning vegetation, is a dependent variable. So I could not fully agree with Crocker.

Jenny: I mention now these terms dependent and independent variables. They, I think, are the greatest obstacle, and still are the greatest obstacle to the success of the state factor approach to ecosystems and soils, and it includes the misunderstanding--what it means to have an independent variable.

I tried to explain in the original book that independence means that a variable can be independent. It doesn't have to be independent. For example, the soil, and a plant growing on it, cannot be independent of each other. The soil affects the plant, and every other second, the plant affects the soil. So there is no independence. But for the flora, the soil is not going to affect the genotypes of the seeds that I put on or that are blown in.

Maher: Do you mean it is not going to affect the genetic make-up of that particular seed; it's not going to change into an acorn?

Jenny: Whether that soil grows a pine or an oak depends on the influx of the seeds, what comes in, and that's independent of the soil. The seed comes in independently. So I can say that the potential flora is independent of the soil. The seed that enters, its genetics, is independent of the soil, because it was formed before it ever reached the soil. In that way, I could conceive the biotic factor as an independent variable, and I have to have independent state factor variables in order to solve the clorpt equation.* Otherwise, it cannot be solved.

Maher: So you were talking about the biotic factor in so much as it is an independent factor?

Jenny: That's right. It had to be defined that way. All the state factors I had to define--I realized that early--as independent variables so they can change independently of the others. Each one can change--and I knew that right away with the climate. I can go to an area having a given temperature, and a great variety of moisture values, or I can take a certain moisture range--a certain rainfall range--and I can find localities where the temperatures differ a great deal. So the two need not be related. They may be related from the meterological point of view, but that is not what is of concern in soil genesis.

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*The historical postulate that soils are conditioned by the five factors climate (cl), organisms (o), topography (r), parent material (p), and age (t), or s = f(cl,o,r.p,t), is put into a conceptual framework that permits solving the equation when landscape configurations are favorable. Moreover, the approach is extended to entire ecosystems. (Frome Hans Jenny, The Soil Resource, p. 205.)

Jenny: I don't ask, for example, how were the rocks formed down deep in the earth, or I don't ask how the weather is formed or how the climate is formed, how the storms are formed. I merely put registration instruments here and there and I take their readings. I exclude all discussions on how the state factors originate.

Maher: You said earlier on you realized that you had to define the independent variables as independent.

Jenny: That's right, as independent.

Maher: When do you mean early on? Do you mean when you began teaching this course?

Jenny: Yes, before I started to write the book. When I worked on the definitions I wasn't thinking of a book. But while I was working on definitions, that was very clear, there had to be dependent and independent variables. I could see that in statistics. If you had a property depending on several variables, and the variables were not independent, you could never understand what was going on. You didn't know what caused what. But that aspect is not well understood, and people think that when I speak of an independent variable that it varies always independently. No, it means it can vary independently. Very clearly, temperature and precipitation I treat as independent variables. When I ascend a mountain, the two change simultaneously, and in opposite manner, so there they are not independent of each other. I cannot separate these two variables to understand why the soils change as you go up a mountain. That can be done if you can get areas or zones in which the two vary independently of each other.

Defining State Factor and the Ecosystem

Maher: Go back for a moment to your discussion of thinking about the causality and connect that up to the idea of state factor.

Jenny: I think the causality idea I gave up very early.

Maher: What do you mean you gave it up? What did you give up?

Jenny: The idea that state factors are causes. I struggled with the causality principle at Missouri, and especially in Berkeley. In thermodynamics you never talk about causality. I felt this was gratifying, that you could talk about reactions without going into causes and effects.

Maher: What then did you mean by the state factor?

Jenny: I concluded then that a state factor is a variable that helps to define a system, or that a group of state factors define the state of a system. All you have to know are the state factors and you have defined the system. I think I got that from chemistry and physics, and I wondered about its origin all the time, why, for example, temperature and pressure and concentration were chosen. If those are known, the system is fixed, say, for a given solution or gas, and all the properties depend on these conditions. So, I defined the state factors, or soil forming factors, as the independent variables that define the states of the soil system. The drawback of this definition is that the ecosystem is so complex that we don't know how many state factors there are, how many state factors must be known to define soil or an ecosystem.

Maher: Were you aware of that drawback at the time that you were formulating--

Jenny: Yes, that's right. Very early I put in the equation what I call the "dot" factors. These are factors which are not included among the five principal factors, and they pertain to factors I might have overlooked, or factors that may become important in certain circumstances. So I was aware that this equation was open-ended, and the three dots indicate that.

Maher: When did you begin developing the language and the examples of sequences?

Jenny: At the beginning. I called all these solutions of the equation functions, such as the dependency of a soil property on rainfall. Both variables were quantified and you could write a mathematical equation and have a mathematical function. But, in the case of Shaw's family, the San Joaquin chronosequence, we knew that one soil was older than the other, but we didn't know any ages. We could not write an equation. So I called these relations sequences.

Maher: Did Shaw use the term chronosequence?

Jenny: No, no, he called it family. Now I use the word sequences also when both the state factor and the soil properties are quantified. I like the word sequences because the word function is used with many different meanings. Many people in the Soil Conservation Service talk about soil functions, and functional soil properties that

Jenny: function for a beneficial or detrimental cause. Thus, I'm using frequently the word sequences instead of functions, but I use the words interchangeably. My usage is fairly open.

Maher: Can you remember gathering together examples? You were already armed with these climate sequences of precipitation and temperature and from Shaw's family, he had a chronosequence. What about in developing examples for your class perhaps?

Jenny: I went through the literature, and I got hints from various people.

An important thing, I should mention, had to do with the ecosystem. When I wrote the book, I was not aware of the word ecosystem. I was sure that this equation--the soil-forming factor equation--also could be used for vegetation. I had a forestry graduate student by the name of Jack Major, who was interested in this approach, and we had many discussions. I asked him whether he would develop that scheme for vegetation, because I felt I did not know enough about ecology or plants of the United States. I was not familiar with the literature; I felt I couldn't do it and I asked him.

Maher: When was that?

Jenny: It was during the war. He had to interrupt his studies and join the fighting in Italy. When he came back I urged him to make vegetation functions. He had a clear head. He wrote "v" for a vegetation property as a dependent variable and as a function of the state factors. He collected examples in the literature and wrote a paper in Ecology, which has been widely quoted, and he is considered the originator of the vegetation functions. Although he was interested in the thing and understood it clearly, somehow I had a hard time getting him to sit down and write it up. I had to say, "You're not going to get a doctor's degree unless you have this done." This delaying was a characteristic which bothered him later in his whole career. He would read everything and understand things, but he had great hesitations putting his work on paper, although he wrote English very well. Eventually he published an excellent paper.

Then, many years later, and especially in the present book, I put together soil and vegetation and formulated the equation for the whole ecosystem. But that concept wasn't there in 1941. I used the term "the larger system." But it was not as convenient as--

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Jenny: [A.G.] Tansley's attractive word. I think that Tansley defined his ecosystem differently. If I would use his definition, I could not apply the state factor approach. So I re-defined his ecosystem in the Soil Resource, but I don't make a point of it. I don't stress it.

Maher: You learned of his term--when was it published, in the thirties?

Jenny: In the late thirties, yes.

Maher: You learned of his term from your discussions with Crocker, was it?

Jenny: I think so. I might have heard it before but certainly with Crocker. We used it freely and talked in terms of ecosystems.

Maher: When was that, the early fifties?

Jenny: Yes.

Maher: How did you see the term as he defined it and how was that inadequate or is that well discussed elsewhere?

Jenny: Do you mean Tansley's definition of ecosystem?

Maher: Yes.

Jenny: Well, I really don't discuss it because the word ecosystem is used quite widely by different people. For example, most biologists--certainly the animal biologists--when they think of ecosystem, they don't include the soil, and to me soil is part of the ecosystem. To a botanist, maybe he would not consider soil either as part of the ecosystem because that is already outside of the root. So I didn't want to create controversies and argumentation that really wouldn't contribute much. So I simply set up the ecosystem as a finite system, whereas Tansley's system is really infinite. He said it's the organism and the environment together. Well, logically the system and environment must be separated by boundaries and that's what I do. He didn't do that. So if you don't have boundaries you can't really talk about fluxes.

Maher: And you included the soil?

Jenny: I include the soil in the ecosystem--essentially my ecosystem is a piece of landscape. I feel the soil is important because it records the impressions of the organisms much longer than the atmosphere. In the atmosphere you have so much mixing that a plant impression, e.g., oxygen pattern, doesn't last, whereas the soil is stable and records the impact of the plants. Besides, soil contains millions of organisms.

Some Additional Aspects of Writing and Publishing the Book

Maher: When did you realize that you were actually working on a book? When did you decide to produce a book?

Jenny: Factors of Soil Formation? Well, I thought that since I am an average guy and have ordinary ideas, so a lot of other average people might be interested in these ideas. It may also be that students urged me to do it, or my colleagues. Maybe Overstreet had a lot to do with it. I'm not sure now. But it followed right away as I was finished with my deliberations.

Maher: What did you mean by saying that when you were put into the position of professor teaching this pedology course that you were kind of exposed in a window? Do you mean that people in the East who knew you would be hearing that you were now teaching a pedology course and they would be looking to find out what was coming of that? You seemed to feel vulnerable somehow in that position. Why was that position anything different from being a professor teaching just colloid chemistry or some other course?

Jenny: No, it was not. But in the field of pedology you might say I was just a guy in the ranks, and then, becoming a professor at Berkeley, I was taken out of the ranks and I was teaching--telling--people what I think is significant in this field. So I assumed a new role. That's what I meant.

Maher: Wasn't that the same thing as when you undertook to teach the colloid chemistry course?

Jenny: Yes, but in teaching colloid chemistry I was not outside of the mainstream. I was part of colloid science. I was not an outsider. I never questioned the principles.

Maher: So you had not simply been "in the ranks" as you put it, but you had some ideas which were not a part of the mainstream?

Jenny: Do you mean in pedology?

Maher: In pedology.

Jenny: That's right.

Maher: Could you be more specific about how you were different, how you felt?

Jenny: I was different and it had to do with the ideas of soil. I met Marbut early, and I had always been a great admirer of Marbut, but I could not understand why he collected only A horizons and B horizons and not C horizons. As I said once before, he did not think that

Jenny: the work Wiegner and I did on ion exchange was pedology. He didn't think that my work on potassium and sodium was pedology. So he had, I would say now, a confined view of what soil is, and it applied to his whole staff. The feeling I had was that the soil surveyors were always on the defensive against the soil chemists and the soil physicists and that they formed a closely knit group, with esprit de corps, because they have been so often on the sidelines.

Maher: You're speaking about the soil survey people?

Jenny: Yes.

Maher: They were in the field a lot.

Jenny: They were in the field and they had to dig. Their symbol is the shovel, a spade, an auger--and they always emphasized the physical work, which is required in what they are doing. They would say, unless you had been digging holes for ten years, you know nothing about the soil. I had discussions with Krusekopf in the field about what are soil properties. He would say soil properties are those properties that you can see or feel. Properties which you cannot see or feel are not soil properties. I argued, "What if I look at soil with a lense or under the microscope?" He said, "No, those are not the properties we want." "What about if you wear glasses?" Well, that he would admit, you could wear glasses. With these discussions, you end up in nirvana.

They also had the idea that soil is an independent body. They wanted to treat soil as biologists view organisms, as entities, and geologists view rocks. A hard-rock geologist doesn't bother about vegetation and animals. So they wanted the soil completely independent, and described by its own properties. I'm all for the latter. That's okay, but it shouldn't end there.

The opposite approach of connecting soil with environment, and seeing the soil as a part of an ecosystem, that is to me also valid. In order to understand soil formation you have to consider the environment, and that's what I did, and that's what they objected to. I wrote a manuscript for Soil Science on the relationship of climate and clay formation and the editor of Soil Science sent it to Washington for review and they turned it down with--what shall I say?--almost insultingly. So the editor--I knew him--turned the manuscript down (1935).

Maher: Why? What did he state?

Jenny: He said he took the judgment of the people who oppose it.

Maher: How was that opposition stated?

Jenny: They said it is an article which should not appear in Soil Science.

That it should go into a journal on ceramics. I could not understand it, so I took it up with Bradfield, and Bradfield had a very strong influence with some people and he wrote a sharp letter to the editor and defended this paper, and the editor then accepted it.

Maher: What did he say that convinced the editor?

Jenny: I never saw the letter.

Maher: Why did Bradfield feel it was so important? Was it simply because you were his student and he had the power to do it?

Jenny: I was not his student. I was his colleague. Well, Bradfield liked the approach. Bradfield had, after all, a science background and had a broader view than most field people. Even now, field people are confined too much, and miss out a lot, and don't get the recognition they really deserve. They are competent and are doing very important work, but being tied to the A-B-C mold and not having enough outgoing to other sciences, they confine themselves.

Maher: Do you feel that if their conceptual framework was more developed, if it had been handed to them, that it would have wider application?

Jenny: The problem is, you see, can they do it? I would say many individual soil surveyors, like Arkley, who came out of soil survey, are very broad and all that. But Arkley is in a university. He can speak out and publish, but a regular soil surveyor is with an agency, such as the Soil Conservation Service. He is not being encouraged to publish a paper, to do research, or so. Only the higher-ups can do that. It is essentially the circumstances that keep people confined. Also, in the earlier days, the field people didn't have much scientific training and didn't have advanced work. It would be so that in summertime, Storie would send his field crews to northern California and in wintertime he would send them down south and they were surveying all year long, whereas in Missouri or in the Middle West, especially in Illinois they are in the field only in the summer. When it snows in wintertime they are in the lab and doing lab analysis, and it's a richer experience than they have here. Now this has changed. We have now at Davis a soil analysis lab and all that, but in the early days it was very one-sided.

Maher: So it was mainly in this way that you felt yourself kind of vulnerable and maybe needing to defend your position in a solidified way?

Jenny: Yes, probably it was a combination of defense and curiosity or the desire to understand the whole thing.

Maher: How did you get it published?
Jenny: I sent it to McGraw Hill and their consulting editor was a botanist

and he turned it down. So again I took it up with Bradfield.

Maher: Why had he turned it down?

Jenny: I don't know. He said it was not suitable for their publication series. Somehow Bradfield changed his opinion. Bradfield lived then in the East, and you have greater advantages in the East. If you are at Illinois, or, especially, at Wisconsin or New York, where he was, you have many more contacts with decision-making people than you have here. We are still isolated, and it's not easy to get travel funds unless you are high up, or an atomic scientist, or have lots of grant money. But otherwise, you don't meet many editors. You don't meet these people. This is an advantage of the East.

I remember when Waksman retired, he came here and visited me and was wondering whether he should come back to California. He was quite impressed with everything, but then he told me, "No, you don't have the connections which you have in the East." He said in New Jersey he can go to New York anytime and talk with important people and the people who make decisions. That's still so, and Hilgard was especially complaining that he lived in exile and didn't have the contacts with the important people.

Maher: Have you felt that way?

Jenny: I could see it, but I was more impressed by the tableau of soils we have here in California, so that never bothered me. Yes, I would say yes.

Maher: What decision making would you rather have been a part of or where would you rather have had more influence? For instance, I notice that you were on the committee in the Soil Science Society for the choosing of terminology. Do you feel that if you had been closer to those people that you may have exerted more of an influence?

Jenny: Not on those, but I think it's more my personality--that I'm not as outgoing as Bradfield is. I'm not criticizing anything. I know if I would look say, for job opportunities for students, or for new developments, or so, you are better off farther East, where you are right down at the ground level and you know all of these people personally.

Maher: You mentioned in your write-up that you felt that the soil survey catechism of the A and B horizons was too narrowly circumscribed though you never mentioned it. Why did you not mention it?

Jenny: Now, I'm talking about what it was then. They have changed a lot.

Maher: I mean back then why did you not--

Jenny: I guess it was from the criticisms I recieved that I was trying to

be erudite and all that. Also, I was favorably impressed by what they did, what they accomplished. I was impressed by that, so I did not want to undermine them, or criticize them, or make them feel inferior, or anything like that. So I didn't bring it up. I couldn't say to my friend Krusekopf, "I wish you would be broader." That would make no sense. It would just hurt his feelings.

Maher: I think we can end there.

Jenny: We took too long again!

Reactions to the Book

Generally Approving

[Interview 12: July 29, 1980]##

Maher: I wonder if you can now go into some of the reaction that the book, The Factors of Soil Formation, provoked or failed to provoke.

Jenny: It had an immediate effect on some people, but not necessarily among soil scientists. For example, shortly after it was published, I asked for a Guggenheim fellowship and I submitted as a piece of record that book. One member of the committee was Linus Pauling. From another member--I don't want to mention him now--I heard that the book made a very favorable impression on the Guggenheim committee, and I got that grant very easily. Later on, after several years, I took it and went to Colombia. So that was a concrete thing.

Maher: When was that?

Jenny: That must have been in '43, but then the war came and I couldn't go. So I postponed the trip until '47, but they kept the grant going or waiting, whatever you might call it. I think the reaction was very favorable abroad, moreso than in the United States, especially remembering that I never got much reaction from the big group of American soil scientists in soil conservation activities and with the soil survey group. In fact, Crocker thought they were deliberately ignoring it. But I had many favorable responses from soil scientists at the universities and experiment stations.

The approach was also well received because it dealt with fundamental questions that were usually not being discussed in the literature, but they were brought up in the book. Also, I was told
Jenny: that the extensive review of the literature was favorably received.

Others commented about the style of the book, that it was--what did the fellow say now? I don't recall the words, lucid or something. So it was easy to read.

Maher: Who was that?

Jenny: I think that was a fellow from Florida, as I recall. Allison, I believe it was.

Maher: The organic matter man?

Jenny: No, I don't think it was the organic matter man. It was the [R.V.] Allison who wrote the book with Bennett on the soils of Cuba. I could see in references in textbooks or at meetings and discussions, that my book also made a qualitative impression, namely that people became more aware of the constellation of factors. People were interested in comparing soils, say old soils and young soils, but they had not appreciated the requirement for the constancy of other factors in making such a comparison. They made a comparison which really was not valid, taking samples from different slopes or different parent materials, simply because one soil was older than the other. I think that was a broad result which clarified the thinking.

It also pointed out that climate functions and time functions were related, that all of these factors were really integrated and that they could be sorted out. That was appreciated by many people. The people in Iowa went heavily into separating the sequences in the state of Iowa--toposequences and biosequences. I could see that workers at other experiment stations would also use this approach.

Maher: Can you give some example where your book was used?

Jenny: I would have to think a bit. Indiana, I know for sure used it, and Illinois, too. I am just trying to think who would have been involved in it. Yes, I guess Guy Smith. Yes, Guy Smith. He told me that the center of pedology suddenly shifted to California from the Middle West where it was before. Iowa, Illinois, and Indiana, in a way, were the center of a lot of important research.

Maher: Can you expand on that, what Guy Smith said and meant?

Jenny: Guy Smith mentioned it to me once and later on he and others--W.H. Allaway, F.F. Riecken--wrote a paper on the soils of the Middle West and I could see the scheme was working in them.

Maher: Who were they--including Guy Smith?

Jenny: Guy Smith wrote a famous doctoral thesis (1942) which was based on the distance function of loess material in Illinois, but I cannot say that the book was responsible for that. However, Guy Smith and I discussed a lot of things at Missouri. So I would say that it had an impact, although he may not have explicitly acknowledged it. Then somewhat later the people in Michigan published a chronosequence. People in Missouri published climosequences. It's been around, all right, and abroad, especially in New Zealand, the school of Walker did a lot of exploring of the functions. In fact, Walker wrote a lengthy paper published in the Quarterly of Biology. It discussed chronosequences, the pros and cons, and he assembled a lot of them that had been published by that time.

The book helped in initiating new courses and new directions of teaching at universities. The new topic, "soil genesis and classification," appeared in course catalogues. Also, my two honorary degrees had references to functions, as did David Bellamy's recent TV series (BBC) "Bellamy's New World," (1983).

Then I noticed that it got into textbooks, at least a discussion of soil forming factors. More space was devoted to them, although I would say, in the first decade or so, very few used any equations or used the clorpt equation. Most people were uneasy. Most people who were in field work were uneasy about equations and did not say anything, not because they necessarily disapproved, but they did not feel competent to handle it.

Another example of the impact on the book was in the seventies in a paper by a fellow from Israel, [Daniel H.] Yaalon, a paper about whether or not the functions were valid, and he also counteracted some past criticisms. But I remember he mentioned at the beginning that the book had an enormous impact, stimulated worldwide interest, and I know the book was translated and published in Russia. That's the only translation I know of. The book is quoted all the time and, wherever I traveled abroad, my name was always associated with the book. That may be simply the reason--not what the book says, but that you have written a book. That impresses many people.

Maher: What do you think was the initial conduit for the reception of the book in Europe?

Jenny: How it got there?

Maher: Through what channels--was it through the regular distribution of literature or did some of your colleagues in Zurich perhaps, knowing your name, send for the book?

Jenny: I never got any reaction about my papers in Zurich. After Wiegner died and younger people came in, the situation changed and especially one fellow, who is now a professor there, Bach, used it

Jenny: in his thesis and mentioned it prominently (1950). More recently, in the early seventies, an ecologist in Zurich (Gigon) used it as a basis of his work and published, interestingly enough, quite a number of illustrative diagrams showing the interaction of state factors in ecological studies.

Mixed Reactions, Generally Not Approving

Jenny: The book was received with mixed feelings in Australia. One positive reaction was from Crocker and that's why he came over here. First for a visit and then permanently. The leader in the soil survey field was very critical about this book. What was his name now? He is a very tall fellow. His name was Stephens. He elaborated on it and put in a lot of differential equations which, to me, showed he didn't quite get the whole point. He confused the independent and the dependent variables and he tried to prove that the approach is not very helpful.

Maher: In other words, he took it as a basically mathematical exercise and carried it along a certain mathematical avenue and made it unfruitful and said then, "It's unfruitful."

Jenny: He published a paper and, yes, what he published was a mathematical exercise and I felt it wasn't productive for the very reasons I gave. Mathematically, you have X and Y, and you can shift them around as to which one to declare the independent variable. But that's not so in nature where you do actual things. You can make a choice which one is more likely independent; you wouldn't switch, say, soil nitrogen and rainfall, and designate the latter dependent.

Criticisms came from various sides, some not very significant, I thought, and others perhaps pertinent. Now, one great criticism came from Crocker. After he was here for some time, he published a paper about it, especially the biotic factor. He never told me about it when he was writing it, but he thought the idea of constancy of factors except one was inadmissible. So he said, "There are really no single functions in nature. They are all multiple functions." I viewed his stand as a lack of appreciation of the idea that an independent variable or a constant factor are relative terms. It is constant to something else which varies a great deal. Then you call that one a constant, but all the things vary. So those are misunderstandings, I would say, of the more fundamental issues about variables.

Maher: How did it come that Crocker had not told you about that, about his publication?

Jenny: He didn't tell me when he wrote it; only when it got out, he gave me a copy. I don't know why he didn't.

Maher: What were your relations like? It seemed to me as though you had a lot of discussions.

Jenny: Yes, we had. But in a way he was a strange person. I don't know why he didn't. I don't know whether he was worried I would talk him out of it, or whether he--I don't know.

Maher: Was he much younger than you?

Jenny: Yes, oh yes, he was much younger. He was very ambitious and he pulled a lot of wires. He did a lot of things sub rosa so to speak, but we need not get into that.

A good point was raised by Sjors in Sweden, an ecologist.

Maher: Can you spell that please?

Jenny: S-j-o-r-s, I think, and he was concerned about the biotic factor, namely that if you had a mutation and a new species evolves in an ecosystem, how are you going to handle that? He did not give an answer, but he felt negative about the biotic factor. But I feel nature is so varied that some of these questions have to be analyzed when they come up to decide how are you going to handle them. You don't have to spell out all of the possibilities in the constitution. We have lawyers to argue the fine points; and you will have pedologists and ecologists who will argue how a special case is to be handled in the light of the broader aspects.

When I retired, I got quite a few letters, that were not solicited by anyone, stating how much the book was appreciated.

Maher: This was by students?

Jenny: No, professors at other universities. I would say that many statements I heard were in appreciation of the book. I guess some of the honors which I received had to do with it. So I would say it was a positive thing in spite of all of the criticism.

The Soil Conservation Service Soil Survey Staff

Maher: You mentioned at the beginning that the soil conservation people and the soil survey people in the United States were not really showing much reaction. One of your colleagues even suggested that you were deliberately being ignored. Could you expand on that, on their possible motivations?

Jenny: He said I was being ignored. In Washington, on one or two occasions it was mentioned, but in a negative way.

Maher: How so? What sort of negative sense?

Jenny: I think someone wrote, maybe it was even Guy Smith, or someone from Washington mentioned it, saying the whole thing was a personal viewpoint. That means I didn't go through channels. Also, the emphasis in it on soil genesis wasn't really in their cards and so it detracted from--it was a sort of a competition with--their work on classification.

Maher: Was it because, in the sense they went by palpable visible field factors?

Jenny: They were not concerned with soil genesis. They were mapping soils and classifying soils. That was their goal. By going into genesis, and coming up with a lot of questions of definitions and all that, they really were not prepared for that and really didn't want to tackle it because they knew their scheme was in many ways not thought through in depth and they didn't want to open that can of worms. This is till true today.

Maher: Had you had much input into the several approximations that have led up to the present soil taxonomy?

Jenny: I don't know of any. See, it is difficult to say. In the book I mentioned soil temperature and soil moisture as cardinal soil properties and that was denounced. But now in the new taxonomy, Guy Smith makes this an important point. They put in the water balances and temperatures. Now, they are measuring soil temperatures and soil moisture regimes. However, I cannot claim that these two events are related to the book. They could be but they may not be. After all, the SCS has many competent people. Why shouldn't they think up the same thing, too, independently? So I don't know how connections or red threads that go through things could be demonstrated.

Maher: What were your relationships with Guy Smith over the years? You met him first in Missouri?

Jenny: I met him first in Missouri, yes, and we worked together a lot and I think we had very good relations that lasted for a number of years. In fact, we wanted to appoint him on our staff and we had a position open. I have forgotten now whether it was Crocker's or Birkeland's. We had him come out to California and look around. I think we made him an offer, but he didn't take it. One reason he said was he thought by being with the federal government he could travel more and there must have been also other reasons. But anyhow, he did not.

When he started to work intensely on soil taxonomy we lost contact and hadn't much communication, but a year ago he was here. He was traveling through San Francisco and had a day's lay over. He came from abroad, so he spent a whole Sunday here and "visited his old professor," as he put it, and we discussed a lot of things.

But at an earlier time we both agreed that our philosophies had diverted enormously, or perhaps more our common interests had dissipated, he with his taxonomy, and I with root-soil boundaries and ecosystems.

Maher: When did you have that discussion?

Jenny: That must have been in the sixties, the early sixties or so.

Maher: Could you sum up the differences?

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Jenny: He became a very strong advocate of the notion that soil classification should be restricted to soil properties, but I never had an argument with him about that. However, I think that by bringing in factors relating to particular soil states, more can be added and also by his push to measure soil temperatures. I think that state factors will come in and field soil science will be enriched by his emphasis on measuring soil temperatures and water budgets.

Guy was critical of my work in nitrogen and carbon temperature functions in the United States, pointing out that in the North the soils are much younger than in the South, which is true, and he thought that must have a big effect. But he had not made the necessary study, and I said, "That effect can be sorted out; there are enough places where it can be shown that you have a temperature effect and that the soil age is not the important thing."

Maher: If we can go back, I believe that you mentioned Guy Smith speaking to you in two contexts. When we were talking about the Illinois and Indiana usage of your basic function idea and yet not giving cognizance, you said at that junction that Guy Smith had mentioned to you that there had been a shift of the center of soil science to California.

Jenny: Oh, a shift in pedology to California, yes. Yes, that's what he said.

Maher: So that was a friendly comment to you.

Jenny: Oh, yes, there were lots of friendly comments, and at many national meetings we would sit together and drink a beer and discuss things. No, there was nothing like a personal animosity or anything, no, not at all. We were great friends, but things happened later in the families.

Maher: Was he kind of tipping you off that there was a certain maybe mild envy or resentment in the Illinois and Indiana group that there had been attention shifted to California?

Jenny: No, he never said so. He made it as a positive statement. However, I did not agree with him on that, though the center may have shifted temporarily because of the book. Really more basic soils work was done in the Middle West than in California. Guy Smith was a very independent fellow and I would not say he was a part of the early mentality of the soil survey group. However, for his classification to succeed he needed the cooperation of a large number of people, so he maintained friendships with soil surveyors in most states, and they appreciated him because he was very good, very profound and had a lot of knowledge. But I would say that he was an independent person and he probably turned the whole thing around during his decades in Washington.

Maher: Can you explain that statement a little bit more, that he probably turned things around?

Jenny: [pause] Under Whitney, the emphasis was to map areas and publish maps as rapidly as possible and, therefore, get support from Congress and get this survey going, and build up his empire. Marbut was a thinker and tried to understand the soils and classify them. He lacked close communication with the soil surveyors. I could often see in discussions at meetings a certain tenseness between him and the soil surveyors. Guy Smith continued the trend of seeking a broader conception of soil survey, and he stressed more rigid thinking. He pushed the plan of running analyses and establishing laboratories to get quantitative criteria, and of thinking about processes. In that way he changed the soil survey to a very positive force in American soil science.

Maher: So he had the profoundness and the tendency to develop a conceptual framework similar to Marbut, but he also had friendly relations with the survey people. Is that correct?

Jenny: That's right and, of course, through me he knew a lot about the Russian and European theories, so he was not stuck in it like Marbut was. What I felt he lacked in Washington is the kind of people I had

Jenny: here such as Overstreet, who would criticize what others were doing. Guy put up these categories and there seemed to be no one there who would dare to challenge him, but this is a speculation on my part. When he first presented the seventh approximation at a Berkeley seminar, I questioned him about slopes, because I had a strong belief in soil continuities on slopes, and so I asked him, "How many of these new soil names do you have along a slope?" He said, "About three." I said, "There should be more since it is a continuum." He said, "We didn't explore that sufficiently." That was his answer.

Maher: When was this?

Jenny: That was in the early sixties. By the way, talking about functions, there was a famous soil scientist in Great Britain, G.W. Robinson. He wrote a book on soils that was well received. He discussed the question of functions, based on what I did, and he thought soil continuity was really a new idea that could be helpful. Also, in that connection there was another well known Britisher who reacted favorably to what I did, and he published it in a travel report in 1940. His name was Milne and he was the fellow who developed the concept of the catena. Recently, in print, a British ecologist called the soil-forming factor book a "minor classic."

Maher: I was going to ask you a question when you were speaking about Guy Smith. Do you feel that perhaps one of the reasons that he did not come to Berkeley at the offering of an appointment may have been that he was accustomed to an insular situation in his position in Washington and that he sensed that he would be exposed to more shake-up and rigorous criticism if he were on this campus?

Jenny: [pause] I would hesitate to assume that this was an important thing. In the end, I heard, he became quite dogmatic. He had a falling out with [R.V.] Ruhe when he and Ruhe couldn't agree on something and so he finally fired Ruhe from the Soil Conservation Service. Ruhe is a very prominent geomorphologist.

Additional Background on Research and Teaching Which Led to Writing the Book

Maher: Now, I understand that the publication of your book had been done initially to organize your ideas in a stable way so that you would have a foundation for your teaching in pedology.

Jenny: That's right. That was for the teaching of the course.

Maher: Now, you were a professor with your own work and with graduate students eager for work. This may have also had a theoretical foundation since it was basically a theoretical work and it postulated on all of these possible sequences lined up with holding everything but one or two factors constant. So you or somebody certainly had a lot of work laid out for you in terms of empirically filling out the sequences. How did that work begin or did you view it that way?

Jenny: At the beginning, at Missouri, I went through many of the experiment stations' publications and found nitrogen data, and arranged them in curves, and a lot of people criticized that approach. They said I did nothing; I didn't produce new data, just arranged old data in a new perspective. But that was not the reason why I went soil sampling. I simply wanted to get more functions. When I got to Berkeley, I wanted to know what the situation was in California. I wanted to know if what I had found in the Middle West was more or less a coincidence, was a valid thing. I felt I wanted to stick with functions, and whenever I had a chance, I would follow it up.

Interestingly enough, I got all of this credit for the nitrogen-carbon work, but I never had an official project for it. It was always done sort of underhanded. I got a little money from here and there, but I never had what is called an experiment station project on that topic. My long time experiment station project was on colloidal properties of California soils, but I had no hesitancy in considering these functions with nitrogen and carbon (organic matter) as dealing with colloidal properties.

Many people wondered, especially Professor Flaig in Germany, and also, most recently, Eldor Paul, why I never went into organic matter studies--fractionations of fulvic acids and humic acids and others. First of all, the experiment station had no interest in humus; the very productive valley soils in California had little of it. Hoagland had hired Professor Barker to teach microbiology and go into humus work, but he didn't. He went into radioactive work and then left the department to join biochemistry. Then we hired Doug McLaren to work on humus, and he didn't do it either. I didn't want to do it myself because it requires a special person, an organic chemist, although I am not sure now that would be the best solution.

Maher: Why?

Jenny: If a fellow is an organic chemist or is trained as an organic chemist, especially with a Ph.D., he has developed, unconsciously perhaps, a loyalty to organic chemical problems and to him, working with humus and soil organic matter doesn't look promising. You might say there is no Nobel prize in sight.

Maher: It wouldn't be rigorous enough?

Jenny: They simply wouldn't know enough about its significance to agriwulture and forestry. That was true with McLaren. McLaren knew a lot of organic chemistry and physical chemistry, but until his last few years, he would never say he was in soils. When people asked him, "What department are you in?," he would say, "Plant nutrition." He had no feeling for soils and especially no outdoor feeling of soils. So that is the problem. If a fellow has too much training in another field, he may not be emotionally involved with the soil problems.

Maher: So what would be the correct alternate approach in your mind?

Jenny: In my mind it would be that a fellow starts in with some soil courses and then goes into chemistry and gets advanced degrees in say chemistry or geology or so. Then he has a foundation about soils. Now, in the past, that was a natural thing because many such people grew up on farms, but that's no longer so. They are mostly city people and many don't even know nature.

Maher: So this is all parenthetical?

Jenny: Yes. [tape interruption] One thing, the difficulty was that our soil survey people did not have extensive analytical data. It was the old soil survey scheme where you do all of the field work with field observation. When I arrived at Berkeley they had a modest soil laboratory mainly for mechanical analysis. Esther Perry was running it. The lab was helpful for measuring pH and to check by proximate analysis the texture of soils to see whether texture test by feel in the field was still unbiased. So there was nothing I could use.

Then there was this huge body of data on alkali soils and alkali reclamation from Kelley's group, but again that was not suitable for the functions. So in California we had to start from scratch and I was concerned about the factor relationships. I wondered, "do they exist?" I knew we had prairie soils, dark prairie soils, as those in the Middle West, almost of identical appearance. How could that be? In Missouri we have summer rains, in California winter rains, that is, enormous differences in the distribution of rainfall and in temperature. So I got Barshad interested in that contrast, and I think his was the first thorough, pedological study that was done after I got here. He discovered vermiculite in soils.

So gradually we built up information. And then came to question about the tropics; how do the functions appear in the tropics? So we made a transect from near the top of the Sierra Nevada to the valley floor and clear to the equator (Colombia). Stan Gessel was helpful in setting it up. I did this, essentially, to see for myself whether or not these functions have broad validity. Also, I was always hoping that I would get additional hints and suggestions

Jenny: about practical problems of soil management. That was always in the background of my thoughts. While that aspect was successful in Missouri, the functions hadn't produced anything here.

For one thing, all of the successful California agriculture was on the valley lands, on the level lands, not in the hills sections where grazing and forestry prevailed. But, whenever students came and showed interest in functions, I would discuss it with them. With Klemmedson, we were concerned how soil fertility related to the state factors, why some soils were fertile and others not and can fertility be intregrated into the functions. He conducted that mammoth pot-test experiment on the Oxford tract where he handled up to a thousand pots; a fantastic trial.

Maher: Was he a good organizer?

Jenny: Yes, and, of course, it could be done only in the Berkeley climate where you have a rainless summer, because the pots had to be outdoors. You would never have a greenhouse big enough to run it, and all the many soils had to be run at the same time. So, to me, that was very gratifying. Already, at that time, we ran into difficulties with air pollution. During hot, humid days the sensitive lettuce plants would be damaged. As a result of that, the greenhouses were then provided with air that had passed a carbon filter to eliminate pollution. That was long before the idea of pollution became prominent here.

Maher: Your book acted as a kind of an organizing structure for a lot of the work that was done subsequently by yourself and your graduate students along the lines of these various sequences. Did it serve for you as a broad structure within which you were able subsequently always to lodge whatever work you were doing?

Jenny: It provided a framework, but it was also a pleasurable thing for me that I could show that the soils in Colorado were linked to the soils in Missouri by a function. To me, that was a gratifying thing by itself. In other words, that the mosaic of soils over the world, or over the American continent, could be shown to have a web of relationships according to the clorpt function. To me, that was a pleasing thing. Yes, when I was in the field looking at soils with my colloid chemical background, my soil chemistry background, I saw things going on there, and vice versa, when I was thinking of the contact theory or other things, I had in mind also the functions where that might show up.

Maher: Could you expand on where the contact theory work might fit into that scheme?

Jenny: I thought especially it would play a role in the heavy textured soils, in the clay loams and the clays. I think there is a validity from the work of the Indiana people, from Stan Barber, that in many

Jenny: soils the highest percentage of uptake is done by what he calls diffusion. In these soils, in the heavy textured soils, I thought there may not be enough transpiration stream to serve the plant and so the contact would be a helpful thing in getting ions into plants, but I didn't come up with any function correlating the contact theory with state factors.

Transcribers: Michelle Stafford, Joyce Minick
Final Typist: Catherine Winter

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The tapes have been deposited in the Microforms Division of
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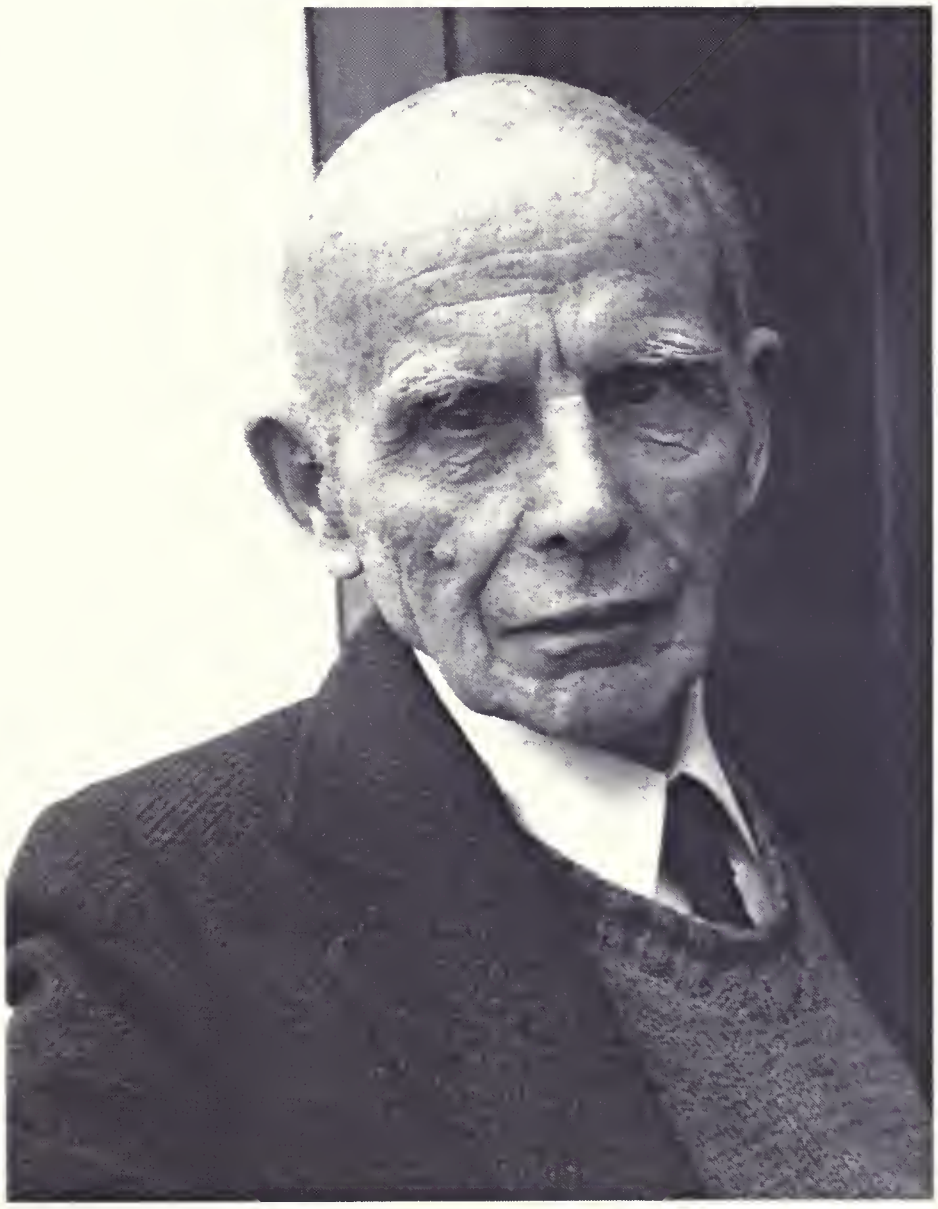

HANS JENNY
ca. 1983

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KEVIN STUART INTERVIEWS HANS JENNY

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I GENERAL OVERVIEW OF SOIL SCIENCE AND SOIL SCIENTISTS

[Interview numbers one to four: September 14, 1983]##

The Development of Hans Jenny's Interest in Soil Science

Stuart: Would you tell me how your interest in soil science developed?

Jenny: My initial interest was more in animal husbandry. I went to the Zurich College of Agriculture and took the course in soils. I had a stimulating teacher in Wiegner who was a world-famous colloid chemist and soil scientist. Though he had no practical knowledge of soils he painted a remarkable picture of the soils of the world.

When I was finishing my Ph.D. in colloid chemistry, a representative of the Rockefeller Foundation talked to me. He told me about a program they had inviting young European scientists to spend one year in the United States. I applied for and got this fellowship. I spent one year in New Jersey. At that time--1927--there was the first meeting of the International Congress of Soil Science in Washington, D.C. Bradfield was there as a professor of soils from Missouri. He decided to take a leave of absence to go to Zurich to study with Wiegner. He invited me to take his place in Missouri. M.F. Miller was head of the department. I stayed in Missouri for several years.

Stuart: Would you describe the development of the Soil Science Society of America (SSSA)?

##This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 327.

Background on Several Soil Societies in the United States

Jenny: In 1927, when I came to Missouri, everybody belonged to the American Society of Agronomy (ASA). At the same time the soil surveyors were not happy in that environment and started the American Soil Survey Association (SSA). They held meetings just before the agronomy meetings. The first meeting that I attended was in 1927. I presented a paper. I found this group very active intellectually. They were interested in many things beyond the mapping of soils and soil analysis. A segment of this group wanted to establish a real society. The leaders of the ASA were worried that this new soil science society would wreck the agronomy society because the agronomy society was dominated by crops people. They fought the development of this soil survey association. And they were in a way successful because all the people that belonged to the Soil Survey Association also belonged to the ASA. But over the years the demand strengthened to have a soil science society.

Bradfield was instrumental in creating this society, not as an independent society but as a sub-society of the ASA. To make this logical, the agronomy people were forced to create a crop society. Now the ASA consists of the crop society and the soils society.

When I was in Missouri, I didn't like this situation at all. I was strongly in favor of an independent soil science society. I had come to believe that soil is a natural body and an object of science. But the people who thought as I did lost out in the vote. Some years later I changed my mind because I had come to see the value of the broader objective of soil science study as a part of the ecosystem. That really developed strongly after I came to California.

My background in Europe and Missouri was with agricultural soils. You plow the land and all you can see is the soil and then you plant the crops and what the crops do depends on the soil. That goes back to the days of Liebig. But already at Missouri, when I worked on soil nitrogen and organic matter, I realized that the roots and the litterfall in the forest are the prime source of organic matter. I talked about "the larger system" which I conceived as the vegetation and the soil. In California I began to study soils of natural or nearly natural forests and grasslands and I became more and more aware that the focus is the larger system that began to be known as the ecosystem.

Well, the old ASA really dealt with agricultural ecosystems. And so I changed my negative attitude about the ASA. I think it is more basic to agriculture than either of the two societies alone. I hope that the ASA will become the scientific leader of eco-

Jenny: agriculture. I think what is needed is for the ASA to become more explicitly aware of the ecosystem concept and to formulate its broader policies on that basis.

Another way of looking at it is that the ASA rarely deals with the ecology of agricultural systems. I think they do not play enough of a role in challenging the people who criticize agricultural research for not doing more work in, say, molecular biology. Too many chemists, too many botanists, look at plants as an isolated body. But the plant, as I see it, functions only in a given environment as a component of an ecosystem. The agronomists are aware of that but they do not conceptualize it or use it as a conceptual tool to support their work.

Stuart: What role did Bradfield and [Emil] Truog play in the development of the SSSA?

Jenny: In the early days soils people were not so technical. Soil physics, chemistry, and biochemistry, have developed since then and many of the people engaged in these fields today have no relationship to agronomy. Many of these people did not want to be under the auspices of the leadership of agronomy people who had essentially a crop production viewpoint. They dealt with practical questions of crop production. Both Truog and Bradfield worked hard to eliminate the SSA and to create a soil science society under the umbrella of the agronomy society.

I have made peace with the agronomy society but many have not. The result is that these people publish their work not in soil science journals but in other scientific publications. In fact they start new journals which dilutes the unity of soil science.

To illustrate what is happening, an Australian--Leeper--published a paper arguing that soil science doesn't exist anymore. It has been split up into other fields with different names such as geochemistry or what we have in our department--soil biology. My hope is that a core of soil science will remain.

I tried with my 1980 book to integrate soil physics, soil chemistry, soil biology and soil genesis into a field of soil science.* But I'm not sure that it will be continued by others at a non-elementary level of soil study and knowledge.

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*Hans Jenny, The Soil Resource, Origin and Behavior (New York: Springer-Verlag, 1980).

Factors Influencing the Study of Soil Science in the United States

Stuart: What in your judgment has been the importance of the idea of soil as a medium of crop production to the development of soil science?

Jenny: In Western Europe and earlier in the United States interest in soils was dominated by its role in crop production as a means of growing plants. The early fundamental work by Liebig and J.B. Boussingault was characterized by this mental approach: how do plants grow? They started analyzing soils, plants, and animals and out of it came the field of agricultural chemistry. Chemists became intensely interested in agriculture, and agricultural chemistry originated. In fact, the first book that I wrote with my former professor was a manual on agricultural chemistry. We discussed soil analysis, feed analysis, wine analysis, and milk analysis. That was agricultural chemistry.

Hilgard was essentially an agricultural chemist. These colleagues were the first scientifically trained people who studied soils. More recently, the change in Europe has come through the influence of the Russian school. The change in the U.S. came through the development of the soil survey under Whitney. That eventually brought in the ideas that we now call pedology--the understanding of soil profiles in the field. And, incidentally, the international success of Wiegner was based on the fact that he combined the Western European school, the Russian school and the American school as represented by Hilgard. He was a great integrator. And I would say many of the older leaders of present day soil science were strongly influenced by Wiegner and had studied with him, e.g., Marshall, Bradfield, and Russell.

The way we look at it, Wiegner didn't know soils. He couldn't feel texture and never used a spade--it was all conceptualization.

Stuart: What were the influences of the soil survey on soil science in the U.S.?

Jenny: In the early days when the United States Department of Agriculture was established the key people were the animal and crop people. The crop people established the Bureau of Plant Industry [BPI]. The soils people had no home in that bureau. Therefore, an aggressive man like Whitney, who had definite ideas about soil and the needs of soil to help the agricultural settlers, could do a lot. He had enough political clout to establish the Bureau of Soils which was fought continuously by the BPI because they maintained that they should study soils as a substrate of plant growth. That conflict still smolders today. It exists within the federal Soil Conservation Service as competition continues between the crops people and the soils people. When I came to the U.S. I was impressed by the animosity between the two old bureaus in Washington.

Jenny: In contrast, the state supported experiment stations were early in explicitly recognizing the importance of soils work. The federal people were behind the times. This lag is covered in the Hilgard book. When I wrote about it I traveled around and interviewed people from the early soil survey period. When I came to Berkeley in 1936 I found a profound cleavage between soils and plant people. At that time I joined the Department of Plant Nutrition. They were astonished there that I had made contact with people in the Department of Soil Technology.

There were a number of people doing work with soils at the early state experiment stations: for example, Werner Snyder in Minnesota, C.G. Hopkins in Illinois, Eugene Hilgard in California, F.H. King in Wisconsin.

According to Hilgard, Whitney felt that only Washington, D.C. should do soils research and, if the states did research in soils, they should do it under the direction of Washington. When I traveled from Columbia, Missouri to some other states to collect soils and do soil analyses for some of my research, the people in Washington opposed it saying that people in Missouri should do work only in Missouri.

Stuart: There were Ph.D.s being granted before the turn of the century. What do you think the dominating concept of soil study was at that time?

Jenny: Hilgard granted a Ph.D. to W.P. Kelley who became a great authority on alkali soils and soils of arid regions. I would say that the field of agricultural chemistry very early got a place in European universities and the great leader bringing this about was Liebig. He wrote a famous book on chemistry related to agriculture that really was on ecosystem chemistry, in modern terminology.

At Berkeley there was Hilgard, who had a doctor's degree in chemistry from Heidelberg and who worked with graduate students. In Minnesota, Wisconsin, and New York, I assume that they had courses in agricultural chemistry. I would say that it really began in the land grant colleges and universities (LGUs). Once the LGUs got going in 1862 agricultural chemistry became a subject to be taught. Later agricultural chemistry split up into animal work, soil work, and plant work. I don't know when this happened. In Connecticut they were doing some early work in animal metabolism. They were not concerned with soils. But maybe M. Rossiter would know more about this. There is a cleavage between California and Connecticut over who established the first experiment station. Rossiter maintains it was Connecticut. I vote for Berkeley.

Hilgard had a big fight with the LGUs, especially with Michigan which was teaching students how to plow and hoe. Hilgard opposed this. He said that students could learn this at home. He

Jenny: thought that students should be taught why to plow and be taught what plowing does to the soil. The development of agriculture at the LGUs was loaded with politics. It had much to do with the agricultural constituencies and what power they had and what they wanted.

Jenny: Hilgard thought he could have Whitney fired and marshalled a group of people to oust him. He failed.

Stuart: Why did Hilgard want Whitney fired?

Jenny: Hilgard thought Whitney was (1) unscientific and (2) he considered him to be too aggressive. Whitney would visit California and then later publish ideas that he had taken from Hilgard and present them as his own.

The states that did a lot of basic research in soils were the ones with outstanding people. W.D. Atwater, in Connecticut, proved something about animal nutrition. It had something to do with respiration. If you had some outstanding people at a college then they could affect the entire college.

My own book on Hilgard had very poor sales. I had to publish it myself. McGraw-Hill said agricultural people don't read historical books. After it was published, some publishers said they would have published it had I sent it to them. I think that there are very few references to that book.

I remember in Zurich, in courses that Wiegner taught, the name Hilgard was very important. When I came to the U.S., nobody had heard of Hilgard until I came to California.

The Aesthetics of Soil

Stuart: Would you tell me what the basis is for your appreciation of the aesthetics of soil?

Jenny: I don't really know what is meant by "basis." Since I have no training in aesthetics as such, I would say that soil appeals to me because of: (1) Feel--I like to feel the soil. It is a pleasure to make a texture test with my fingers. I like to mold a soil--particularly a clay soil--which is a short step from ceramics. But I like the feel of it and I know children do. I think the art aspects of feeling have not been sufficiently developed. (2) Sense of smell. The soil has a fragrance. I like to sit on the ground and experience the fragrance of soil. (3) Seeing soil. Then what impresses me is the visual aspect of the soil, the soil profile, the colors. I remember when I was a student in Zurich I read works

Jenny: by the famous German poet Goethe who wrote something about the "Sicilian red earth." I took a trip one year to Sicily to see the red earth. Even when I was in the Alps, or wherever I worked, I enjoyed the colors and the combinations of colors and the sculpture of soil profiles. Also, the different structures of soil horizons are very attractive. I know too, soil is attractive to many soil surveyors but they rarely mention it. It is not conceptualized. (4) The shape of soil masses. I also think that the shape of the horizons is very attractive.

In another part of this oral history I go into the artistic significance of the soil. I come from a family of craftsmen--wood carvers and painters. Whether that has a relationship to it I don't know but I think it does. I like to go to art museums and I like to sketch.

If you think of soil as "dirt," as most do, then you won't expect to see any beauty in it. This idea is a barrier. Other languages don't have negative connotations such as "dirt." I worked on French farms, and German farms and I never heard a corresponding word. I read that the origin of "dirt" was not in England.

Stuart: How would you explain the lack of aesthetic appreciation of soil on the part of many soil scientists?

Jenny: I think that you would have to ask the soil scientists that you interview whether or not they had a farm background. I feel that a farm background leads to a positive attitude about soil.

Stuart: Do you see any conflict between considering soils from a scientific point of view and being able to appreciate the beauty of soil?

Jenny: I think it has to do with our educational system. I would like to look into how it is done now in Europe. I wonder if the attitude is different where the educational system is different. I have a problem myself with how much time I should spend on these things. Hilgard once said (he was a pioneer in the scientific approach), "I'm getting tired of being a missionary on missionary pay."

I would say it goes back to there being less interest in art in the U.S. than in Europe. But I don't know how it is in Europe now.

Perhaps, also, science is so fascinating and consuming that there is little need for an aesthetic approach or for considering aesthetic aspects. I don't encounter the feeling of being an oddball. I think it is because my attitude about soil as an object of art has been published by the Vatican, which gives it standing.

The Evolving Understanding of Soil and a Conceptual Framework for Soil Science

Stuart: How do you define science?

Jenny: It is a way of looking at nature and thus it deals with sense impressions, hence I would not include logic and math as sciences. There are two broad fields in science: (1) systems analysis, and (2) reductionism. Both of them depend on curiosity: trying to understand nature and your environment--trying to understand what is going on. Why it is the way it is, in terms of systems analysis and reductionism.

Stuart: What is soil science?

Jenny: I would say that science has to do with ideas, theory, and hypotheses. For example, I wouldn't call the new taxonomy a scientific piece of work, or the description of a profile. It is no more scientific than a cookbook recipe, unless the facts are tied to theories.

Stuart: What would you say Soil Taxonomy is based on?

Jenny: I don't see any guiding principles. To me it is not made clear why there are ten orders and not nine or twelve. I don't see any scientific background. It is very "specific" but not scientific. It is a bureaucrat's delight. But I don't have anything better to offer. It is better than what we had before.

G.D. Smith developed it. He was one of my students. He got an M.S. with me. When I was head of the department at Berkeley, I offered him a job to come here but he said no. I think of Marbut and Smith as two thinkers in soil science.

Soil Taxonomy is the product of a group effort. Smith had to make a lot of compromises. The product contains a conceptual background but it is not spelled out. It's not spelled out why clay is used as a high level criterium and not pH. They go back to an English philosopher in their philosophy of taxonomy (J.S. Mill), but I'm not sure that he was right.

Smith tried very hard to conceptualize it but it got started for practical reasons. Smith got his M.S. under me at Missouri and later went to work for [Charles] Kellogg as a soil coordinator. He told him he couldn't do this soil coordination work until there was a better system of soil classification. He convinced Kellogg that the soil classification system at that time (1935) was illogical and not good for doing a good job of coordinating soils.

Jenny: Kellogg said that soils should be classified by their own properties. I agree with that fully. They also say that soil is an independent body. I think that is a fallacy. They have come a very long way. For example, when I published Factors of Soil Formation, it was criticized by soil surveyors who said that soil temperature and moisture were not soil properties. I was severely criticized for saying that they were. Now, under Smith, temperature and moisture are being measured regularly. But the relationships of soil properties among each other are not being conceptualized. We don't know how many properties are needed to define soil. And many soil properties are self-correlating and others are not. For example, pH and pOH are, and the amounts of carbon and nitrogen are well correlated with each other. There is the problem of the existence of millions of soil properties: how could surveyors pick them out, and on what grounds? It is not discussed. I feel that this is a serious handicap because taxonomy doesn't give us any lead toward seeing a linkage to other sciences--chemistry, physics, or biology. Soil is defined as an isolated system. We have no logical connections to other fields and yet they must exist.

Then, what isn't discussed is that some soils vary continuously from one soil segment to another (there are a series of small gradations) but the whole system is based on discontinuity--separate soil individuals. It is not discussed how this is logically balanced so that you have a logical system--like the periodic system of elements versus diffusion parameters.

It is easy for me to criticize but I couldn't bring up anything better. This goes into the question of the philosophy of soil science. I would say that many of the problems are also inherent in ecology. But ecologists spend more time and effort in clarifying the conceptual aspects.

The question which does interest me is the genesis of soils, how they form and how they evolve. And one big question is what does nature have in mind? What is the fate of soil? With animals and plants, we say that evolution improves or advances, making more complicated organisms, higher organisms. But with a particular soil, what does nature have in mind? But if someone were to try to publish a discussion of it I don't think it would be published in the soil science journals because there is no place for these kinds of questions.

Stuart: What plan would you say nature has for soils?

Jenny: Looking at the various time functions that are taking place: if there are no disturbances, like an earthquake, then the evolution, in terms of biomass production, would go from zero to a maximum and

Jenny: then would decline. If a soil in the humid regions stayed in place long enough it would become more acid, it would lose nutrients, it might form pans, and it would become less valuable agriculturally.

This question came up a number of years ago when I wrote Factors of Soil Formation. It was translated into Russian and the translator said that Jenny was the symbol of reactionary capitalistic Anglo-American soil science. Why? Because I discussed chronosequences (time functions) and I said soils reach a steady state. This idea is contrary to Marxist principles and dialectical materialism: the steady state equals stagnation and death. I took this criticism seriously. I studied Marx and Engels in the original and also read some of Lenin and Stalin to see if this was really true. I concluded that it wasn't. The soil state is steady as long as the state factors don't change.

Stuart: How important do you think the idea of domination and control of nature is in science?

Jenny: The control of nature is the result of understanding nature. Look at astronomy, the oldest science that we have. It is based on observation. We can't control the sun, moon, and planets but we can understand their ways and out of that knowledge we achieved control of timing. We understand nature according to certain concepts. Most scientists try to understand nature according to ions, atoms, enzymes, and proteins. Our society wants it this way. But in the Middle Ages the understanding of nature was in terms of religious dogmas.

Ions, atoms, molecules are really concepts that you can't comprehend with the senses. It is a reductionist approach. You reduce nature to small parts, study them and then try to integrate them to understand the whole.

Difficulties Inherent in Defining Soil

Stuart: What would you say serves as the dominating conceptual framework for soil science? Have there been changes over time?

Jenny: During these many decades of research in soils the ideas about it have shifted. The concept of soil, the meaning of soil has changed from a purely utilitarian point of view. The idea that soil is an independent natural body has been a premise in soil science. This idea originated in Czarist Russia with the Russian school. Mueller in Denmark in the 1870s and 1880s discussed the idea of A, B, and C profiles. The Russian school pushed this idea. Dukochaev and others were scholars who had a broad idea of a soil body and of soil and the environment. They were the first to establish a soil

Jenny: forming factor equation. Somewhat earlier Hilgard had the same ideas in a way better founded, but he did not conceptualize. The Russians discovered the climatic effect on organic matter. Hilgard discovered the effect of climate on inorganic matter. This led to the idea of soil as a natural body--a body that deserves to be investigated for its own sake.

In the United States these ideas gained a foothold in the late 1920s and were strongly fostered by Marbut. But Marbut really had a static approach. When he sampled soils he would sample only the A and B horizons because he said the C horizon was parent material and not really soil. As a result there are lots of chemical analyses that can't be used in soil genesis because the data for the C horizon are missing.

I like to think that this concept of soils as a natural body still prevails. You, Kevin, would know by your interviewing people, if this is true. I would not know. There is a tendency now to not look at soil as a whole body but to focus on certain subelements. One group of people, for example, is only interested in selenium. Geochemists concentrate on a few elements and the same narrowness of soil interest is also true of many soil zoologists and soil microbiologists.

I don't know how the people at the boundary regions of soil science look at soil. Ecologists, for example, look at soil as the substrate of plants and rarely go to any depth in their soil studies. I find the least appreciation of soils by zoologists or at least those who study vertebrates. People who study invertebrates have more of an interest in soil. But to me to look at soil properties, to at least one meter or two to three meters of depth, is the domain of soil science.

Stuart: How would you explain the decline of the concept of soil as a natural body?

Jenny: I'm not sure that it has declined. Soil taxonomy has been built on this idea.

Stuart: What is soil?

Jenny: I say in my book on the soil resource that I don't give a definition because there is no definition that has universal consent. This is frustrating not to be able to define the subject that you work with. But biologists can't agree on a definition of life, so this inability to define the basic thing you are working with is not unique to soil scientists.

In each study each person has his own definition of soil but it is not explicitly defined. I think you would get an idea of how a person defines a soil by how he samples a soil--how many horizons.

Jenny: Joffe, in his book, has a definition of soil. In the 1920s and the 1930s I think there was more emphasis on the definition of a soil than there is now. If you take Joffe's definition and take out the word "soil" it becomes an acceptable definition of the sea, hence it is not specific enough.

I define soil in the soil forming factor book as a body on the surface of the earth that is affected by its initial state (parent material), its age, and its environment. If I got deep enough, where environmental temperature and rainfall have little effect, then this definition would exclude very deep soils. Soil is sensitive to environmental factors. This definition conflicts with laymen's ideas about soil.

Any rock on the earth's surface will also be a soil because rocks have a temperature gradient determined by the environment. This view is logical though unconventional because the geologist calls the soil a rock. Rock is soil if its properties depend on the environment.

Soil as a Living System: Man's Role in Disturbing and/or Preserving

Stuart: What meaning or value does soil have for you?

Jenny: You are really asking two questions. The meaning would refer to soil as the upper portion of a land form. The value is that many soils are life sustaining bodies. If I look at areas that have only hard rocks there is not higher life. Thus I feel that conventional soil is important and that I have a relationship with it and that one day I will return to the soil whether in full body or as ashes. I feel I would not wantonly destroy a natural soil, yet some compromise with society is unavoidable. For example, I don't approve of what happened with the gold dredgers in the Sacramento Valley. There you had the destruction of a life producing body--the death of a soil, merely to extract gold dust.

Referring to the Red Hills in California, I would find it difficult to reason with people like the American Rifle Association who want a piece of land for rifle ranges and large parking areas, with people who want more swimming pools, et cetera. The decision on what will be done with the land, whether destroyed or preserved, will be a decision based on political expediency.

Stuart: What do you mean when you say soil destruction?

Jenny: In the case of the gold mining you started with an alluvial land which had a substratum full of cobblestones. Above it was mainly a loamy soil. Dredgers turned the whole thing upside down and sieved it. When they were finished, you had nothing but a cobblestone land surface. A new parent material had been made. And the soil as a life-nursing body was gone.

There is a viewpoint among economists that if you have a resource, such as soil, that you should use it up and create something better; e.g., a piece of redwood forest is cut and a redwood church is built in its place. The conventional opinion is that you have improved on nature. This is a very deep-seated notion in the human mind. I'm part of this value system too. My house is built of redwood so it was right to cut the redwood down, though I never thought of that conclusion until this moment.

Stuart: You have talked about soil as a living body. Why is that important to you--to include this aspect of soil in your definition?

Jenny: As you say, I don't call soil an organism, I call it a living body. An organism reproduces but soil doesn't. It can't make two soils where there was one. Soil is a living body in the sense that if I were to measure its metabolism, the soil would take up oxygen and give off carbon dioxide. Soil respires. We need soils to maintain the human race.

Some might argue that we could build large barges and live on the sea surrounded by soil-less land. But the sea needs soil on the land in order to survive. The sea is being nourished from an eroding land source that helps to preserve sea life.

Stuart: What is important about calling the soil a living system?

Jenny: If you have pollution to the extent that life in soils is dead, then our life will end too.

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Stuart: If we define soils as a living system--as a living body--and we have respect for that quality, then what criteria do we use to deal with the soil--to perhaps cause harm to the soil?

Jenny: If you talk about harming soil you are thinking anthropomorphically. We identify with soil if we talk about harming soil. If I look at a pristine ecosystem, I could argue that any disturbance of vegetation or soil is harmful to the soil. I would rather not use the word "harmful." I would like to think or even work for the idea of preserving soil areas in their natural states, which means that the soil may not be disturbed or, at least the disturbance by humans is to be kept to a minimum or, better yet, reversible. And the question comes up: why do we disturb it? We do it because we

Jenny: want something from it. In the great majority of cases we want to grow crops. And by doing so we will change its properties and especially its living components.

Depending on our philosophical point of view, our view of nature, I see two alternatives. (1) The biblical and the evolutionary point of view (e.g., we are the peak), that nature exists to satisfy our wants and needs. (2) That we are part of nature and should try to disturb it as little as possible. The first point of view gives us a free hand to use soil for whatever we want, not only for the mode of crop production but to move soil material around to make highways, to build earth dams, to dump it into the ocean, whatever we feel like doing. The only restriction might be that if the soil disturbance damages our well-being we would change our tactics. I would say that the predominant viewpoint of soils is the first one. And our concern for the soil or to be in favor of the soil is to keep it producing food and fiber in an economical way.

Stuart: What do you mean by disturbing the soil and how does that differ from harming the soil?

Jenny: Disturbing the soil has no psychological connotations as to what we think of the soil, whereas harming the soil implies that we already assume that the soil has an identity of its own and we are comparing it to ourselves. It is really difficult to see how you could harm an object unless you endow it with an emotional content. If I drop a stone in a pond it never occurs to me that I'm harming the pond. But I would be disturbing the surface of the pond. It doesn't involve the notion of good or bad. But in my own thinking I have a relation to the soil. I would think in terms of harming it. I think this discussion really touches on ethical, philosophical and religious problems of how to think about nature. And also perhaps according to progress in science because the boundary between animated and non-animated bodies is not clear.

Sometimes I think of soil by comparing it to a city with all its complicated makeup and its components and in the same way I would consider the city as a living ecosystem. And I also take a personal view by using expressions such as "I like it," or "I don't like it."

There used to be, on this campus, a professor of psychology who studied the psychological reactions of people to their environment and their responses to various landscape features. I have heard nothing about him for many years.

Stuart: What does it mean to disturb the soil?

Jenny: Cultivating the soil as a gardener, using a spade or as a farmer uses a plow and disc. I think of this action in a positive way. And I can do it in such a way that I don't destroy the soil, keeping it as much as possible in its original composition. I see it occurring in nature, like a deer eating grass and shrubs, without saying that it is "harming the ecosystem."

If I disturb soil, I could say I'm harming the existing ecosystem. I realize I'm switching back and forth with the role of man. At one moment I think of man as a part of the ecosystem and the next moment I think of him as an enemy in the environment of the ecosystem, as in man against nature in a nuclear war. But man, as a good farmer, is pro-ecosystem and pro-soil.

Stuart: What is your personal relationship with soil?

Jenny: In my younger years as a child, I used to walk barefoot quite a bit and I was very conscious of what I was walking on--a plowed field, red mud, or warm earth. I had close contact with the substrate I was walking on. But I don't walk barefoot anymore. In fact, it hurts me if I do. I love to see a plowed field with the furrows and the many colors and the worms and other organisms crawling around. Yes, I guess unconsciously I'm thinking of the phrase, "Healthy soils make healthy people," and I would like to think that good soils make good people but that is definitely not tenable.

It would also be interesting to talk to a psychiatrist--to ask what he thinks about soil or how it affects his patients. In fact, not too long ago I read a book by a psychiatrist who was studying Indian tribes. I don't recall whether he used the word "soil."

Influential Leaders in the Development of Soil Science in the United States

Stuart: How would you describe soil as interpreted by the discipline of soil science?

Jenny: Wiegner is one case of a person looking closely at soil and Bradfield was another with a different approach. I see soil in a third way. You will see Wiegner as I see him. To him, soil was an object for application of colloidal chemical ideas. I think Bradfield had a view of soils as a body that provides food, not only for him as a farmer, but for humanity in general. I would say that Liebig had the same approach as Wiegner--in trying to apply the ideas of chemistry to soil. But he had a wider perspective than Wiegner in applying concepts of soil to humans. To Wiegner this was of no concern.

Stuart: How would you describe your own approach to soils in your work over the years?

Jenny: At the beginning, when I was a graduate student, I was asking myself whether it would be meaningful to devote my energies to the study of soils. I wasn't thinking of a lifetime spent looking at soils. One of my undergraduate classmates was horrified that I would even consider that because he thought I would only devote my energies to an extremely specific field. Now I would call him an agriculturist. He was an educated farmer who knew a lot about everything in a positive way--field crops, cattle, entomology. So to him soil science was a highly specific element of agricultural studies. I also asked an older friend--a woman who was active in humanitarian activities--and she encouraged me to pursue that field.

However, it is not to be misunderstood that I was just standing there looking for an answer; I was already studying soils. The pH theory had just come up and related analytical questions. So I was already immersed in studying soil out of sheer curiosity. I would say now or perhaps always, especially since I came to Berkeley, I felt like apologizing for studying and teaching soils because the field is not appreciated in an urban environment. When I go to parties and people learn that I'm a professor of soils they are surprised. They don't know the field exists and that applies even to faculty people. And they sort of feel embarrassed as to what conversation to start with me. If at all, they talk about their gardening experiences. So being a soil scientist has no image in the public's mind.

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Stuart: Would you describe Marbut, Truog, Kellogg, and F.H. King and the impact they had on American soil science?

Curtis F. Marbut

Jenny: Marbut had a very profound influence in that he changed the old system of soil classification by introducing the European concept to the United States, especially that of the Russian school. And to Marbut the unification of soils was a great experience. He was in charge of surveying all the soils in the U.S. and Hawaii. The old system of classification by provinces had no scientific content in that he could not understand why certain soils in California and the Midwest looked alike and behaved alike. I'm thinking now of the grassland soils. The Russian scheme would give a hint that this had to do with climate. The whole climatic notion of the Russian school impressed Marbut and helped him arrange U.S. soils in a broad scheme.

Jenny: When I presented my first paper in 1927 on the effect of climate on organic matter of the soils of the Great Plains area from Canada to southern Texas, he was greatly pleased because, as I could see later, it provided quantitative support for his ideas. Ever since that time he has been very friendly toward me and helpful. Thus, when he came to Missouri on a visit--he grew up in the Ozarks--he would look me up and even take me on field trips around Columbia and show me various landscapes. He had an intimate knowledge of the landscape features of Missouri. There was, however, one unfortunate aspect of our relationship because Marbut was hostile to chemistry. He viewed the application of chemistry to geology and soil science as unfortunate and, since my work tended to stress soil chemistry, our contacts became less frequent and less intimate. Nevertheless, I admired him as a prototype of an outstanding American person.

So what Marbut did was to change the system of classification and to organize a monumental piece of work--the first large scale soil map of the United States. It was and still is a tremendous piece of integration of the work of hundreds of soil surveyors and it impressed a wide segment of the public--such groups as geographers, ecologists, fertilizer people, agronomists, journalists, and writers. I had these maps mounted and framed into a large map which I had hanging in my office and it was always a pleasure to look at it and appreciate the broad soil patterns of the U.S.

Emil Truog

Jenny: Truog was at Wisconsin when I met him first. He was friendly right away because he said he also had a Swiss background. In fact, some years later he went to the Swiss Alps and looked up the village from which his father or grandfather had come. Truog to me was more of an agronomist than a soil scientist, although he was very progressive in soil studies, especially on such questions as applied soil acidity and particularly the liming and cation exchange properties of soils. I'm not aware that he was interested in theoretical soil science. He was active in growing alfalfa and his soils work concentrated on improving soils to get good crops.

In a way I should say Truog was a shrewd politician. He was very active in the American Soils Association and he opposed the tendency to separate soils people from the agronomy society. He had a great influence on the U.S. because many students went to Wisconsin to get an M.S. degree or a Ph.D. and later these students occupied important positions in many American universities. At meetings I remember students would praise him and emphasize that they were products of Wisconsin and Truog.

Jenny: In complete contrast, Hilgard had very few students that developed into leaders on the American scene. I remember in the 1930s that the main centers of soil science were Cornell and Wisconsin. Missouri was just coming up through Bradfield and Bradfield brought an entirely different style of soil research as compared to Truog.

Charles E. Kellogg

Jenny: As to the impact of Kellogg: Marbut died on a trip through Asia--in China. I remember we had a memorial service for him in his home-town, in his newly built retirement home in the Ozarks. I don't remember if his body was there or not. In that large living room his soil maps were hanging on the wall. After the service we discussed who would be his successor. The Missouri people thought it might be M.F. Miller. I don't remember the details but suddenly there appeared on the scene Charles Kellogg, a young person who had been a professor of soils at North Dakota. By what strategy and manipulation he became Marbut's successor, I don't know. He brought with him most of his North Dakota graduate students. They occupied, for decades, leading positions in soils work.

I remember the first meeting at which Kellogg was introduced and that he talked about his plan. He smoked a pipe and while he was talking he kept the pipe in his mouth. The impression he made was that he was arrogant and conceited. I never took to him and our relationship remained formal. But no doubt Kellogg had a great influence on pushing soil survey activities and popularizing soil science. He wrote a book entitled The Soils That Support Us, and he discussed Marbut's system of soil classification. It was a well-done popular book and must have had a real impact. But I don't know how such impacts are really assessed or collected.

In a way the professional life of Kellogg was a tragedy because, no sooner had he taken over the soils work in Washington, than H.H. Bennett promoted the SCS [Soil Conservation Service] and began a different method of surveying the soil landscape. For decades this led to heated and prolonged fights between Kellogg and the men in the SCS. Kellogg surveyors were considered to do more accurate and fundamental work, whereas Bennett's surveyors stressed soil mapping from the viewpoint of soil conservation. This controversy spread to most of the states and to California. When I was head of the department here, we had constant conflict and friction between Professor Storie, who was in Kellogg's camp, and the California SCS people. In the end, a compromise was reached. Kellogg's group was fused with the SCS but the surveying techniques became those of the Kellogg group. Kellogg died a few years ago shortly after his retirement.

F. H. King

Jenny: F.H. King was a contemporary of Hilgard. They worked together fighting Whitney and had similar ideas about the role of science in agriculture. King stressed what is now called soil physics, but to the wider public he is known for his account of the Chinese farmer. He traveled in China and was impressed with how the Chinese farmer, with the use of nightsoil, was able to maintain the fertility of his land for thousands of years. King was concerned that in the United States the soil resource would not last very long because our farmers were not treating the land the way other nations were doing.

I never met King. He died before I arrived in the U.S. He died before Hilgard. Hilgard in one of his letters to one of his relatives lamented the death of King, who was one of his supporters, and he said that King was in his sixties and died too young. All I know about King is what I heard from others and what I read about him and from his writings.

Jenny's Criteria for his Research

Stuart: What criteria did you use for selecting your research topics?

Jenny: At the beginning, I was influenced by the teachings of Wiegner and what he thought was important. Those were the topics I was interested in myself, except that early I developed an interest in field work. On weekends, I would go to the surroundings of Zurich, to the farms, and select a plowed field and collect soil samples, put them into paper bags, put them on my bike, take them back to the lab and determine acidity and the amount of calcium carbonate. One problem I was faced with was how to document the collection of a dozen or so soil samples from a plowed field of about one acre in size. At that time, I had no training in statistics. The idea of a random sample was not understood; I had never heard of it. Wiegner couldn't give advice because he never collected samples in the field.

Finally, I decided to collect samples along a straight line--maybe in a diagonal through the field. But, of course, there were many straight lines that could be drawn in such a fashion over a field and which ones had merit, I didn't know. Before long, I began putting these imaginary lines on contours and also in relation to bedrock. I think this initial struggle eventually culminated in selecting samples according to the soil forming factors. And I remember that I was pleased when it occurred to me to draw lines according to the mean annual temperature or mean

Jenny: annual precipitation. In that way I solved automatically where in a landscape I should draw my sampling lines. Later on, I became aware that the proper way of sampling is by collection of random samples along the chosen lines and by combining the statistical approach with a conceptual approach to soil genesis.

Many research activities I developed in response to being asked to participate in some sort of project, as with my experience at Riverside, where I was asked to collaborate on water penetration. Earlier than that I was asked by a famous ecologist--Braun-Blanquet--to participate in his vegetation studies in the Alps. Later on, when I had graduate students, the election of research interests depended quite a bit on what they were interested in. I would give them a fairly free hand as to the topic they wanted to study, but not a completely free hand, because it had to be in an area of my interests and competency. Also the final choice of a research area depended on how it could be financed. For a while easy financing was obtained by working on projects using radioactive substances.

Through that period, my interest in research projects had to do with what I had done previously. For example, soil organic matter and climate occupied me all the time because I wanted to be absolutely sure that the discoveries I had made were valid on a broader scale, and I would encourage my students to repeat this work in other areas.

I went to South America, India, and finally to Africa to supplement and support what I had found before. Likewise, because of my interest in colloid chemistry, I expanded on the work on ionic exchange and the early ideas on plant nutrition. So you might say there was a steady current through my active period of following these lines and of expanding them--to give added support.

I was also interested in practical applications and I had a rule of thumb to spend 20 percent of my time working on projects that would help the farmer directly. I would say that these efforts have been successful and gratifying. For example, the early Missouri work led to the appreciation of soil nitrogen as being a determining factor in the low corn yields of the southern states. The people in the South started applying nitrogen to soil and the yields improved significantly. Before that the conventional explanation was that the low corn yields in the South were caused by dry spells in the summer.

Another practical application was the work at Riverside that led to the non-cultivation of citrus orchards. This helped to improve the rate of irrigation water percolation. Another practical aspect, and this was really collaboration with some work that was basic research, was the development of gaseous ammonia fertilizers for soils.

Jenny: I was involved for many years with the SCS in a systematic soil testing project for California soils. This was related to the theoretical studies of ion uptake by plants. This work in a way electrified the fertilizer industry because now they had a scheme for identifying nutrient deficiencies in soils of California. And as a further consequence of that work, Standard Oil Company built a large fertilizer plant in Richmond, California. I was heavily involved in planning and organizing that enterprise.

Also some of my research ideas evolved during lengthy discussions and associations I had with my colleagues, especially Overstreet, Hoagland, and also Ulrich. So there were very productive associations. I had always more projects in my head than I could finance or find space to do.

Never in all these decades have I been told by my superiors what kind of research I should do. Even now, I would say, that when I don't have an opportunity to do work in a lab of my own, that I still have many ideas and try to induce others to take up these ideas on the integration of practical and theoretical problems.

As you can see, I have bottles of soil samples on my table. I still work on these in conjunction with some people interested in my soils research. Three years ago I went to Africa to collect samples on Mt. Kilamenjaro as an extension of the ideas I developed from my Missouri work. Also, I am spending some time trying to understand what factors or soil properties are responsible for the pygmy forest in Mendocino County.

Additional Discussion on the Contribution of Leaders in Soil Science

[Interview 5: September 19, 1983]##

Stuart: I would like to go back to our earlier discussion. You mentioned at one point that Bradfield had a different idea about soils than Truog's concept or that he had a different approach. What did you see as different, considering the approaches of these two people?

Jenny: I don't know what Bradfield's background was. I wouldn't be surprised if he was a chemist or at least that he had a good background in chemistry. He talked the language of chemists, whereas Truog didn't or only sporadically. I felt Truog wasn't really involved nor that he really knew much about modern physical chemistry--that it was rapidly growing at that time. I think he had training similar to Hilgard's, based more on analytical chemistry, inorganic chemistry, and organic chemistry, so there was a

Jenny: difference in the things the two were interested in and the kind of vocabulary they used. An old common term in Truog's vocabulary was "soil acidity," but I didn't know for sure just what that meant in terms of Truog's soil analysis. The word acid pertained to the strong acids used by the early chemists--the analytical chemists--who analyzed compounds using strong acids, whereas the newer approach, you might say, concentrated on biochemistry, dealing with dilute acids or highly dilute acidic conditions. The key symbol for that view, or that attitude, was the expression "pH" which had been created in Denmark, I believe, before the First World War. When I first heard Truog talk, I wasn't sure I heard him use the expression pH. Bradfield talked about his weak clay acids and "talked pH." So I would say there was a definite but not serious generation gap in terms of chemistry.

C. E. Marshall

Stuart: While we are talking about soil chemistry, what would you describe as being the contributions of C.E. Marshall?

Jenny: I see at least two aspects as far as his work in the United States is concerned. He did some beautiful work while he was still in England, and also in Zurich, but that never reached the American continent. I see two things that impressed me.

One was the explanation of the cation exchange capacity of clays. The first model, based on x-ray analysis of clays, was, I believe, developed in Germany. It was a model of montmorillonite. I think the fellow's name was Hofmann. He placed the seat of the exchange capacity at the fringe of the platelets--the clay platelets--and the reaction of exposed silicon and aluminum ions with water, which yielded an exchange capacity. Although Hofmann discovered and interpreted correctly that montmorillonite has an expanding lattice, he really was unable to account for the high cation exchange capacity.

Marshall, who had a good background in mineral optics and mineralogy, introduced the idea of isomorphic replacement of cations. I presume that he took this idea over from the people who had studied micas. They had the idea of isomorphic replacement, and Marshall applied this idea to clays and successfully explained the cation exchange capacity by pointing out that aluminum may replace silicon, and magnesium may replace aluminum inside the crystal and, in that process, the crystal develops an excess of negative charges which can be compensated by foreign cations, for example, potassium, sodium or calcium. And these ions would be exchangeable.

Jenny: The second striking contribution of Marshall is related to pedology. There was always the problem of measuring gains and losses of elements in the formation of the profile. Rainfall leaches the soil but how much leaching took place in the past was not known for sure because there was no agreed element to serve as a standard. Many geologists had considered aluminum as the most resistant and stable element, which led to the well-known ratios of silicon to aluminum and bases to aluminum--two key ratios. However, pedologists showed that aluminum in soils was not resistant and immobile, that it could move from A horizons to B horizons, and thus the value of the ratio, with aluminum in the denominator, was weakened.

Marshall, influenced by sedimentologists, proposed the study of highly resistant minerals like zircon as a reference mineral. He analyzed Missouri profiles, showing that such resistant compounds did not change in some profiles and that, under those conditions, they could be used as markers and absolute gains and losses of elements could be calculated. To me, that was a significant contribution to soil science.

In other matters, Marshall and I and my colleagues were in serious conflict about the suspension effect. He had an entirely different interpretation than the one we offered. Now, it depends on the future of the suspension effect. If future research shows, proves, or supports that the junction potential is the cause, the Berkeley group deserves the credit for providing a valid explanation. If, on the other hand, future research supports the Donnan Potential as the cause of the suspension effect, then Marshall and his supporters will get the credit.

Stuart: We talked about this earlier, but maybe you could repeat it for the sake of the tape recorder--what the disagreement was between Marshall and Peech?

Jenny: Well, I think I mentioned before that I'm not fully aware of what the controversy was about. I always put Peech in the camp of Marshall as far as the suspension effect was concerned, so I really don't remember what they were arguing about. We were arguing with both Marshall and Peech so I wasn't aware there was really a triangle of controversy.

Roy Overstreet

Stuart: Some people that I have talked to have mentioned Overstreet as being someone that is important in American soil science and important in soil chemistry. What would you say about him and his contributions?

Jenny: Well, when I came to Berkeley, Overstreet was a laboratory technician in the Department of Plant Nutrition. He had just finished his doctoral degree in chemical thermodynamics with a Berkeley Nobel Prize winner. Right away he liked the things I discussed in seminars and he started working with me on colloid chemistry, and when I left plant nutrition to join Kelley in the new Department of Soils, he came with me. We spent many years on the mechanisms of nutrient uptake by plants and we were promoting the so-called contact theory. He was an exquisite analyst. He was so good that he would not run duplicate analyses. He thought that if you had two analytical results and they are not the same, you would have to run a third analysis. Only when the result of one analysis was in conflict with the results of other analyses would he consider running repeats.

Overstreet was active in formulating thermodynamic considerations of the contact effect and of the suspension effect, and in doing so he became interested in the considerations of soil chemistry in terms of classical chemical thermodynamics. This is not surprising, because in the chemistry department was G.N. Lewis, who had developed the activity concept of solutions. After some years of collaboration, Overstreet became a professor and embarked on his own research. He worked on basic questions on ion uptake--and, with Jacobson, proposed the idea of ionic carriers to explain the non-exchangeable uptake of ions by roots. Later he had a student by the name of Ken Babcock, who got a Ph.D. under him and published an important treatise on thermodynamics of colloidal clay systems. Later Overstreet had other students and he did considerable work on alkali soils and their chemical reactions. He died too early, as a consequence of emphysema.

M. L. Jackson

Stuart: What would you say M.L. Jackson's contributions were?

Jenny: Jackson was very active in the earlier days in work on clay crystallinity. He studied clays of many soils and advanced a much quoted weathering sequence of clays. He worked on the behavior of potassium in clay crystals. This led him to contemplations about the role of aluminum in soil and he came up with a broad scheme of the behavior of aluminum in soils. This interest in aluminum was combined with his studies of the behavior of phosphorous. And he developed methods and techniques to separate various phosphorous compounds in the soil and also various iron compounds. Later on, he broadened his research and discussed such things as the influx of dust from the atmosphere and its role in soil genesis, which impressed me.

Jenny: Jackson attracted a lot of graduate students and many of them later occupied positions in American universities. So, in a way, he continued the tradition of Truog in making Wisconsin a training ground for many American soil scientists.

Guy Smith and Soil Taxonomy

Stuart: I would like to return to something you said earlier about Guy Smith. We were talking about Soil Taxonomy and the conceptual base for Soil Taxonomy. You said that Smith tried hard to conceptualize a framework for Soil Taxonomy, but because he was working with many other people, it was difficult for him to do so, or this made it difficult for the final product to show a conceptual framework. How would you describe the framework that Smith had and that he would have liked to have seen become a part of Soil Taxonomy?

Jenny: Well, I would say that Guy Smith did his taxonomy work without my involvement. Perhaps the only thing I could see as a strand or a thread of my involvement is the period in Missouri where he worked with me for a master's degree on clay pan formation. I was giving a course on soil genesis, and we had together many lengthy discussions on soils and soil classifications. So, maybe in a way, this early work served as background material. I was told that the reason Guy Smith came to Missouri--he was actually a student and staff member at Illinois--was that he came to work with me to obtain acceptance for some of his ideas about soils. Or, maybe acceptance is not the right word, rather to have an opportunity to discuss his ideas on a technical basis and certainly that was what we did. I found him to be a very broad and conscientious thinker who always wanted to go into depth. And he was not satisfied with a quickie solution. So usually, when he came up with an idea that he had thought through, I was ready to accept it.

A few years after Missouri, he went back to Illinois and did a Ph.D. thesis on loess distribution. I would say it was an example of the functional approach, which I had begun to teach in my soil genesis class at Columbia. From there he went with Kellogg to classify soils and he told me that he saw right away that the existing Marbut system of soil classification was inadequate. At one time, when I was head of the department here, we tried to get Guy Smith to become a staff member. He came out to Berkeley, visited some soil sites, but finally decided to stay with Kellogg because he thought, as he told me, that he would have a greater opportunity to travel abroad and see the soils of the world than he would have being a professor with a teaching schedule.

Now, I have forgotten really what the question was you asked me.

Stuart: I was asking about his conceptual approach for developing the system of soil taxonomy. Earlier you had said that he tried hard to conceptualize but that he had to work out the system with a number of other people and he had to make a number of compromises. So I am interested in your understanding of what his conceptual approach would have been.

Jenny: All right. I don't know what his specific early ideas were. It was a fact that he only knew the soils of Illinois, but it was relatively easy for him to extend that knowledge to include the entire Middle West. To get soil information from the New England states, or the South, or the West, or Hawaii, he had to go there but also rely on the local soil surveyors. And he felt that the new system of soil taxonomy that would be proposed by Washington would not be accepted by the states unless they had their inputs and their ideas about what their soils were. Guy Smith was interested in fundamentals and he read books about classification because, at that time, many people in the various states had their own ideas about classification. I would mention [T.M.] Bushnell in Indiana and Storie in California, and there was, in general, a tendency to consider biological classification--the taxonomy of plants and animals--the binary system--and to apply that to soils. He was skeptical of it and therefore consulted philosophers and other scientists as to what they had to say about classification in general.

Also there was the problem of nomenclature. For example, the word "podzol" was used worldwide but the soil scientists of various countries did not have identical definitions of podzols. Therefore, Guy Smith was in favor of discontinuing the worldwide terms of soils like "podzol," "chernozem" and "laterite." And he engaged a professor of classical languages to help him coin new words. Also, Guy Smith was strongly in favor of using physical and chemical analyses to describe soils in the field--an attitude that was just the opposite of what Marbut favored.

I had several discussions with him in which I asked questions about continuity and discontinuity in soil classification. He responded that they did not go into these questions to the extent he desired. I assumed there were enough leg-work problems to be decided than to go into the more conceptual considerations. A few years ago, he came to California and called me up and said that he wanted to spend an afternoon with his old professor. We discussed principal questions. I asked him, for example, why he had established ten orders and not nine or twelve. He said ten was a good number but the ten were not fixed in concrete and before long he could see an eleventh order emerging.

Stuart: You said that Guy Smith did a Ph.D. thesis at Illinois on loess distribution and that you would say it was an example of a functional approach which you had begun to teach in your soil genesis class at Columbia. What is the meaning of the term "functional approach"?

Jenny: The "functional approach" is the process of relating soils to some of the state factors along a gradient. For example, the development of the soil at a given site in relation to soil age would be a time function, or the changing soil properties along a gradient of rainfall, as from the Great Basin to the Atlantic Coast, would be a rainfall function--a soil rainfall function. The functions in Smith's thesis dealt with distances from rivers, or the river plains as being the source of windblown loess. He also studied what might be called dust-influx functions, that is, how soil properties change according to the thickness of loess deposits as they thinned out at greater and greater distances from the river flood plain.

Stuart: We talked about this earlier but I would ask you to repeat it for the sake of the tape recorder. How did the ideas of Marbut and Smith on soil taxonomy differ?

Jenny: When Guy Smith started to work on Soil Taxonomy, Marbut had already been dead for several years. Marbut's ideas on soil classification were taken from the Russian school and, in a way you might say, they were classifications according to climatic principles. The theory was that you had zonal soils which were in equilibrium with climate and vegetation, and intrazonal soils that were determined by unusual parent materials and unusual topographies. Marbut was an adherent of the geologist [W.M.] Davis, who postulated that mountains would erode and become undulating landscapes, but Marbut thought in terms of mature soils that would be in equilibrium with the environment.

Guy Smith and others--Washington and the state colleges--were skeptical as to how one could identify a mature soil and how the podzols and the yellow/red soils could serve as broad schemes for classifying soils. So he and others in Washington insisted that soils should be described with their own properties and that scientists should not speculate on what they might have looked like a millennium ago, or whether they are mature or will continue to change.

The Pygmy Forest Research

Stuart: I'd like to ask you how your own work with the pygmy forest has influenced your ideas about soil.

Jenny: When I studied chronofunctions--that means time functions of soil evolution--in the fifties, I studied the Rhone-glacier morraines. That situation was essentially a short time chronosequence embracing a little less than 400 years. I was anxious to look for still longer periods of soil formation that offered constancy of factors, that is, the state factors, except time, that would not change much during that time period. Well, we had a sequence which both Dixon and a colleague of mine--Crocker--studied at Mount Shasta--the Mt. Shasta chronosequence. It was several hundred to a few thousand years old. And I was looking for a time sequence of much longer duration, as it would occur in the tropics, leading to laterite crusts.

I knew about the pygmy forest but I was not sure whether it was a part of a chronosequence or not, and, when Bob Gardner asked me for a topic for his Ph.D. thesis, I urged him to study the pygmy forest. He accepted readily, because, in 1950, he was in charge of a soil survey of Mendocino County, and he had first-hand information about the soils of the pygmy forest. In fact, he published a paper about the pygmy forest and related soils, but there was nothing in it about time functions or other functions. And, when he did his thesis in the late sixties he recognized that the pygmy forest and its spodozol was located in a sequence of marine terraces. That led to the evolution theory of the staircase, but it took many hours of agitated discussion and field trips to formulate and conceptualize this picture.

I have the highest regard for the late Bob Gardner. He did an intense amount of work in a short period of time, but we were not sure to what extent he was right and, therefore, Rod Arkley and I, and Schultz, as an ecologist, checked and rechecked some of his observations and collected additional data to fortify the notion of the staircase being a chronosequence. So this notion made me more interested in the very old soils and what nature intends with soil genesis. This gave me impetus to consider the fate--the future fate--of the majority of our soils.

Stuart: What would you say the future fate is for the majority of our soils?

Jenny: The future fate of the majority of our soils I view in terms of the clorpt equation--the state factor equation. And I wonder about something that brings me back to Marbut when he said that the prairie soil in the Midwest is an equilibrium soil--a mature soil. These midwestern soils are probably less than ten thousand years old and how should we visualize what they will look like one hundred thousand years from now?

But to go back to an earlier comment about what I discussed with Guy Smith on the new taxonomy, I'd say it contains a bias and the bias is that he and many of the participants who were forging the new taxonomy lived in the Middle West and worked with the

Jenny: prairie landscapes. So the prairie landscape and its soils occupy a predominant position in Soil Taxonomy, at least that is the way I feel about it. And that work, of course, is embodied in the soil order known as mollisols, which is a very broad extensive group or order.

II THEORIES AND CONCEPTS OF SOIL SCIENCE

[Interview 6: September 21, 1983]##

The Essential Soil Profile

Stuart: What group of theories or concepts would you say is central to the field of soil science? And then why would you feel those theories or concepts are important?

Jenny: In a way, it would depend upon how we define soil science. Is it the study of soils as a neutral, non-personal scientific object, or does it involve personal experiences? By the latter I mean that we study soil science and develop it to answer practical questions. Do we talk about soil science as an area of study, without regard to the human element? In parenthesis, I should say that I'm not sure that this is really possible. I would say that the central concept to me is the soil profile that was developed independently by the Russian school, by the West Germans or the West Europeans, especially Mueller in Denmark, and very early, I think in the 1850s, by Hilgard, who clearly recognized horizons, although he did not use that word. But he described them and specified their depth intervals. In fact, many of his descriptions are, in a way, natural ecosystem descriptions. So the concept of the soil profile is essential.

We don't all agree on how deep a soil profile should be--how far down we should go. That is because of our different backgrounds. In Europe and in the northern U.S.A., where we had glaciations and many parent materials formed by the last glaciation, the soil profiles are shallow and in many cases it is easy to tell how far down the soil extends and where the unweathered parent material begins. But in most of the United States, especially in the South, where soils have been exposed for a long, long time and weathering has extended to great depths, there are valid differences of opinions as to how far down we should study soil and call the body soil.

Jenny: From this central point of the profile you can choose from two approaches for studying soil. One may be called systems analysis and the other is known as reductionism. Systems analysis views the broad macroscopic properties of soil, what we really can see and feel with our senses. Sense impressions mark much of the activity the soil surveyor uses in the field, whereas reductionism takes the soil apart into many components. This leads to the broad field of soil chemistry and the equally broad field of soil microbiology dealing with bacteria, fungi, actinomycetes. With this biological field I should also include invertebrate zoology because soil holds thousands of species of small animals.

Among the broad principles in soil chemistry I would list the experiment of Thomas Way in 1850 when he percolated salt solution through a soil column, analyzed the solution before it entered the soil, and again after it percolated through and left the soil. Way's findings of the fundamental changes in the composition of the percolated solution opened the field known as ion exchange, and I would say that it is a crucial signpost in soil chemistry. This ionic exchange field immediately embraces the colloidal particles which are the reactive ingredients of soil, and they include the clay particles and the humus particles. These are two fundamental components of soil leading to the individuality of each soil body. Since clay is so fundamental, the study of clays has received an enormous amount of attention and, in the last decades, great progress has been made in visualizing what clays look like.

The other domain of colloidal particles, as mentioned, are the humus particles and I like to think of a chain of events starting with photosynthesis in which water and carbon dioxide are joined to create organic matter, e.g., carbohydrates, and oxygen. And as organic matter falls on the soil, as a leaf falls, or enters the soil, as roots, it is being decomposed by the organisms, forms stable humus substances, or more precisely, relatively stable humus substances, which eventually are decomposed again to carbon dioxide and water. In that process the organisms consume the oxygen that was originally liberated by the above-ground portions of vegetation.

Another area of interest in soil components is the soil solution. Rainwater penetrates the soil and organic and inorganic particles dissolve in it and some people make a comparison or an analogy of percolating salt solution with the pulsating blood in animals. The emphasis on these various aspects of the soil varies with time, with cultural period, and different subject matters are stressed at various universities, but there is an extended world-wide activity trying to understand these cardinal properties and processes of soil.

Jenny: Well, you asked here why are these theories and concepts important? They are important immediately for the things that we want to do with the soil. First, for growing crops, we have to know--have to assess the nutrient capital of the soil--what nutrients need to be added to produce bigger and better crops. Soil physics, which I haven't stressed, deals with the circulation of water in the soil, and is of paramount importance in arid and irrigated agriculture because there water is usually the limiting element in crop production. The role of microbes is more difficult to pinpoint to questions of practicality; they are more a general agent of the life processes, the available nutrients, the decomposition of organic matter. They make these occur. The more food production we want, the more we should understand soil. The more substances we add to the soil as fertilizers, pesticides, or poisons, the more we have to understand these dynamic processes that occur in soil. Soil is not simply a sink for what we might want to put into it. It is a dynamic living system.

Sometimes the question comes up: which soil property is the most important one? Bradfield used to say that pH is the most important one. He would say that if he had to make a choice as to what soil property to measure, his answer was that he would measure pH. Other people would measure other properties. It would depend upon where you are on earth. In the arid regions, you certainly would want to measure the water content of the soil. In areas of red soils, you might stress the available phosphorous. So, I would say the property to be studied depends upon what question you are asking regarding the soil. In my work on soil genesis, the formation of clays and of humus appear as crucial components of soil.

Stuart: In answering this question that I just asked, you said, "I would say that the central concept to me is the soil profile." Why would you say that? Why the soil profile?

Jenny: I don't know how flying birds look at the soil. When they see a bare soil it is just a surface. If they descend on it and are looking for insects or worms, they are interested in, say, the first inch of the soil. Gophers are interested in a foot or two of soil, and man, who wants to grow crops, is interested in the first few feet for truck crops and pastures. He may be interested in a twelve-foot section if he wants to grow a walnut orchard. So the depth of the soil is involved in broad ecosystem considerations, and in root behavior, and the properties of the soil change with depth. So the layers of soil, or its horizons, have many practical consequences. Theoretically, the depth function of soil, or the change in the soil with depth, depends a great deal on the environment and its temperature and rainfall. So the soil depth is

Jenny: important for the theoretical study of soil, and, in essence, the soil profile is the very criterion of how to define the soil and separate it from other bodies.

The Inseparability of the Theoretical and the Practical

Stuart: Would you say then that if we looked at the body of theoretical knowledge that soil science has, then these practical considerations of growing crops are responsible for the structure of knowledge? In other words, sometimes people talk about science as being a consideration of nature without any value, that it is an objective look at what is going on, but you are describing this theoretical body of soil science knowledge as being something that is answering some very specific question: how can we produce the maximum amount of crops?

Jenny: Yes, I would say that the practical questions have attracted interest in soils and certainly in the colleges of agriculture, where soils are taught, the expectation is to come up with answers to practical questions. However, the person who tries to answer these questions and studies soil soon encounters his own questions in trying to understand how the soil behaves. The soil investigator may simply be concerned with answering a practical question; another soil investigator may be concerned with the soil as a natural body, and some times the same person does both. He makes a contribution to agronomy or forestry, and at some other time, a contribution to the understanding of soil, regardless of its practical applications.

As soil knowledge grows, more and more people become what might be called soil scientists who spend most of their time studying soil as a scientific object. I would say, however, that even an investigator who has an agricultural background will see those two aspects fused together. To him the idea of calling one aspect of the study practical and another part theoretical is not a crucial issue. In fact, many of the theoretical soil scientists assume that their research may lead to beneficial, practical consequences. I have had, in my own research, several such interrelations where a purely theoretical study led immediately to broad practical applications of soil knowledge.

I would not say that all research has to lead to practical consequences. I think our western society relies on the knowledge of theoretical ideas about soils and all natural bodies. We encourage scientific research and often talk about scientific truth as a goal of society's activities.

Stuart: In your answer to the first question I asked, you asked in return if we talked about soil science as an area of study without regard to the human element and then you said, "I'm not sure that this is really possible." Why isn't that possible? Or is it possible to study something without regard to the human element? Is it possible to study soils without regard to the human element?

Jenny: I don't know whether it is possible or not, but I would say it isn't occurring as far as I know because, in all research I have been associated with, there is an emotional element involved. When I worked with Overstreet in the early days of radioactive counting, we had to work all night through until five o'clock in the morning. We did it eagerly, because we were curious about the results, because we had certain ideas and we were personally involved in that research. In other words, I would say that pure research or soil research could not be conducted by a robot. There must be brains and blood and flesh behind it. So much in the ideas we have depends on cultural values and cultural expectations in a society--the words we use, the language we speak, our expectations; our disappointments in our results depends on personal opinions. So I can't really see a neutral, absolutely independent science.

Soil Properties, Processes and Behavior

Stuart: You've talked about two aspects of the soil: soil properties and soil behavior. What does soil behavior mean and what is a soil property?

Jenny: I remember having read a theoretical book by a physicist on physics. He was an Englishman. I think the book was called: Physics: The Elements, by Campbell. He discussed at length what is a property in physics or in chemistry. I don't recall his definition, but if you ask me what is a soil property I prefer to give the answer in specific examples. I would say the clay content of the soil is a property, or the humus content, the carbon content, the weight of the soil, the porosity of the soil. I would say there are thousands and thousands of soil properties. In contrast to properties are processes. Is that the word you want to know?

Stuart: Behavior.

Jenny: I would relate behavior to process. How fast water penetrates the soil. How the ions move. How the soil expands in taking up heat. How the soil shrinks when water is removed. How it conducts electricity. So the behavior would be identical with processes, which means relations between the properties of the soil and the exchanges with the environment.

The pH Concept

Stuart: Why did Bradfield argue that pH was the most important soil property?

Jenny: I can answer it only by analogy because he became interested in pH at the same time I did as a graduate student. pH was a new concept and it had great promise for agriculture. I remember the first paper I ever wrote was on pH. I was still a graduate student and the general idea was that it would explain a large number of things such as diseases and actions that we didn't understand. As I, Bradfield had a chemical background, and it was shown through study and analysis that pH was crucial in many biological reactions, in many chemical reactions. And so it was a ready speculation to assume that pH is a very important soil property. Bradfield didn't have actual information for his claim. It was a projection based on his chemical background.

Stuart: How important would you say the idea of pH is to the field of soil science?

Jenny: For one thing pH has an enormous range in terms of numbers: from 0 to 14, at least, which is a terrific range from alkalinity to acidity. Secondly, pH is involved in many chemical and biological reactions in the soil. We can change the pH in directions that benefit the culture of plants we are interested in. The significance of pH is sort of a point of view. We may look on pH as a result of many processes in the soil and say that the measurement is the combination of all of them. But we might also ask ourselves: what are the causes that bring about that pH? In that light pH becomes an effect--a dependent variable that would acquire less significance.

There is a famous case of [Jacques] Loeb and the lyoptropic study of proteins series where colloid chemists had shown that protein systems behave differently depending upon whether they contain potassium or sodium, or calcium or magnesium, exactly as with the clay systems. Loeb, however, claimed that if you adjust the pH and bring all these systems to the same pH then the ion difference disappears. A suspension of calcium proteins or calcium clay has a different pH than a suspension of sodium protein or sodium clay. That way the pH is a dependent variable and you wouldn't stress it. But Loeb pushed it the other way and changed pH and then said that these cations are no longer important in determining the behavior of the colloidal system. And maybe that picture is true for many soil properties. I like to think their importance depends on how we look at that origin and how we view all these properties, including pH, as a function of the state factors and whether we put the emphasis on the state factors, and view properties like pH as the result or the effect.

Quantitative v. Qualitative Analysis

Stuart: If you look at the body of knowledge generated by soil science or if you look at the Soil Science Society of America Journal, it strikes me that much of the knowledge in the Journal is quantitative in nature, and a lot of the knowledge has been generated by computers, pH meters, and whatever. There is not a great deal of qualitative knowledge. Would you agree with that and, if so, does it disturb you in any way?

Jenny: I'm not sure that I can subscribe to your statement because every measurement that we make is really an amplification of quality. Yes, we have more properties of the soil that are quality properties and I assume they are not quantitative properties. Of course, what I think you are driving at is our tendency toward quantification. We tend to neglect properties that have not been quantified as yet. And in that way, soil investigations may be one-sided because you are so much better off with a quantified property than a qualified property. But I agree and that is where system analysis comes in. Many properties--important properties--like structure, for example, are not sufficiently investigated because they cannot be quantified. And, probably, dealing with qualitative properties, like color for example, the challenge and demand on scientific investigators are greater than they would be buying an instrument and starting to measure properties.

I would say that the chemist and the physicist paid attention to quantitative measurements, whereas the biologists are compelled to deal with qualitative properties, or at least, with properties which are not readily measured, like some properties of ants or worms or amoebas or microbes such as to their shapes and forms.

Stuart: Some soil scientists that I have talked to have said that they are interested in some of the qualities that you have just talked about--color in particular--but they are really at a loss as to how to investigate these properties because they can't be readily quantified. What would you suggest to people who have an interest in looking at these things but because they are accustomed to using science in a particular way don't really have any guidelines for looking at them?

Jenny: Well, take the color that you mentioned. I remember when I first came to California and took the summer field course under Professor Storie. We had to describe soil profiles in the field. One problem was that we didn't know what nomenclature to use--what Professor Storie meant when he called a soil reddish-brown or whitish-pink. We had to find out by trial and error what color scheme he had in mind. And this uncertainty was throughout soil science--through all the states. When a soil is described in Pennsylvania as yellow-brown, and in California as yellow-brown,

Jenny: who knows whether they are the same color? So, very early the soil survey association had committees trying to define soil color so we would have a consistent nomenclature all over the United States. The problem was solved by outsiders--by the Munsell color scheme, and the Soil Science Society adopted that scheme for soil description. Now the study of soil color has practically ceased because the Munsell chart and color chips are satisfactory for the needs of soil surveyors. I would say it is a semi-quantitative system.

The Social Consequences of Soil Science

Stuart: Do you feel the knowledge generated by soil science has had any social consequences?

Jenny: The knowledge generated has had enormous practical consequences, enormous economic consequences, and I don't see how these consequences can exist without having a social impact. The very fact that we are able to produce such enormous food surpluses with our modern plant varieties and soil management practices must have affected the social fabric of farmers and of wider segments of society. I cannot see how our shopping habits and their consequences could have developed without the progress in production of food and fibers. I would say a change from working with horses to plowing with tractors must have changed family life and the patterns in the farming villages. So I would answer your question with yes, it has had social consequences, but social consequences is a broad term. Maybe more specific societal questions should be put.

Stuart: One example of a change in society would be the change from a population that was largely rural based to the present situation where the population is largely in an urban area. Could you give some specific examples of how soil science has played a role in bringing about that change?

Jenny: Are you referring to the depletion of the agricultural society, or the fewer numbers of people active in agriculture?

Stuart: Both.

Jenny: There are many factors that have contributed to the problem of having fewer and fewer farmers. How to isolate the soil factor, how much it has contributed, I think would require an extensive study. I don't think I could deal with that. I could not answer it, because it involves so many economic variables. I can't answer this question on short notice.

Stuart: Why did you say so many economic variables? Wouldn't research itself in soil science be greatly influenced by economic variables? Isn't soil science and the kind of research being done in soil science a product of economic variables so when we talk about research--when we talk about the kind of knowledge being generated by soil science--aren't we really talking about economic variables?

Jenny: Well, one criticism of soil science and other phases of agricultural sciences is the paucity of publications that lead to practical applications, and by practical applications, economics to increase the incomes of the farmers is what is really meant. I would say that in lots of studies that go on in California soil science departments--I don't see an immediate relationship to economics. For example, at Riverside, where they do thermodynamic studies of soil solutions, it is hoped that they will have an economic impact later on. But I don't see economics involved in studying the problem in the first place. That problem is the result of advances in chemistry and in physics and in computer science rather than an impact of economic considerations.

The Ecological Consequences of Soil Science

Stuart: Do you feel that the knowledge generated by soil science has had any ecological consequences?

Jenny: It is my belief that soil science is very basic to ecological sciences, but that connection is not sufficiently appreciated, and not enough work is being done by soils people and by ecologists to bring these fields together. I think we are just at the beginning of a cross-fertilization of soil science and ecology. I feel that the delay has been caused in great part by the soil science people in not integrating their important findings with ecological questions. Soils people are not sufficiently aware of the crucial questions in ecology. For example, I feel that soils played an important role in evolution. And I base this on the correlation of many endemic species with particular soils. When I talk to botanists about this idea, that extreme soils might be evolutionally active by changing the rate of mutation, the botanists take a negative view. My soil science colleagues are completely unaware that a relation might exist, and only the molecular biologists see any merit in such a question.

[Interview 7: September 23, 1983]##

Stuart: Do you feel there are any connections between soil science knowledge and the progressive degradation of soils in the United States?

Jenny: My answer would be that degradation continues or increases in spite of the knowledge that soil science has accumulated. Soil science knowledge is, in a sense, in favor of soil preservation, or in favor of viewing soil as a natural body. People who are soil destructive, like farmers who permit severe erosion, or soil mechanics engineers who view soil for building purposes, are not acquainted with the knowledge of soil science such as the soil profile and soil genesis.

Stuart: There are a number of soil scientists that I have talked to--soil chemists, soil physicists, soil microbiologists--who I would guess have very little knowledge of soil classification and soil taxonomy. Do you think that their lack of knowledge of soil as a natural body would lead them to look at soil differently--that they might look at soil in the same way as some of these other people?

Jenny: Yes, I would think so. They would take a more specialized interest in the soil body, like ground water problems for a soil physicist, or litter decomposition of the soil surface. Pedologists consider litter and the decomposed litter on top of the soil as soil horizon, therefore, a soil property. But I talk to soil microbiologists who do not share that view or who do not consider the organic horizon on the surface of the soil as being part of the soil. They like to consider it as an intermediate zone between the leaf zone and the mineral soil. I cannot share that view and I would think that if they worked more with soils, even within these fields, that they could not avoid seeing the overall picture of all the horizons of the soil.

Wasn't there a second part to the question?

Stuart: If there is, I have forgotten it. Let me ask another question along the same lines. Do you think there is a connection between the work soil scientists have done in the area of chemical fertilizers and say, loss of organic matter, increased soil compaction and increased soil erosion, in the United States?

Jenny: Well, I concur that fertilizer applications have brought about soil changes and these have been enumerated and studied by soil scientists who work in the field of soil fertility and agronomy. But the amount of fertilizer added or the broad field of fertilization, I believe, cannot be directly attributed to the soil scientists. I would attribute it to the fertilizer salesmen who go around and talk to the farmers and show them the good results they get and the farmers that follow this advice. Perhaps the university extension fertilizer specialists may enhance this trend by explaining some of the actions and what cautions should be taken, but I don't feel that the soil scientist encourages the use of fertilizers.

Jenny: By saying that I may contradict myself because my study of the nitrogen climate function led to my conclusion that the low corn yields in the South were a problem of a lack of nitrogen, and I know extension agents and fertilizer people used this information to propagandize the use of heavy applications of nitrogen in the South. So I guess one could answer questions both ways, yes and no.

Soil scientists have played a role in the use of fertilizer, including in California. When I collaborated with the SCS in a systematic study of pot tests of California soils, we showed and mapped where the areas are in California that are deficient, very deficient in nitrogen and phosphorous. And I know that the fertilizer people used this information to expand the application of fertilizer in California. In fact, Standard Oil built a fertilizer plant in Richmond, California, based on the results of our pot-test experiment. I would not personally have gone out and urged farmers to apply more fertilizer. The influence of the soil scientist is indirect at best.

Stuart: We talked the last time about soil properties. What do you consider the most important soil property?

Jenny: Yes, we started on that and I mentioned Bradfield's idea that pH is the most dominant property. It depends somewhat on how the question is formulated. If you ask what is the most important property I would ask, "Important for what?" Are we talking about drainage or crop production or erosion? My reaction to the question would be based on my work. I would give a very high ranking to soil organic matter for the question of importance from an agricultural and silvicultural point of view. It would also depend on what factors are missing. If the soil is very acid, like a pygmy forest soil, I would say liming or acidity is the most important property. So really the purpose for which the soil is to be used should be stated.

One could perhaps ask what is the most important soil property for soil with native vegetation on it. Then again the human mind would have to ask the question, "What for?" And automatically we would think in terms of biomass production, but I can't visualize a natural ecosystem having purpose in its life--in its evolution. So the question, "What is important to an ecosystem?" doesn't make sense.

The Natural Ecosystem and the Question of Purposeful Evolution

Stuart: Earlier you had talked about looking at a soil body and asking the question, "What plan or purpose does nature have for this soil as it develops?" Is that a legitimate question and if it is, why wouldn't the question about "purposes" of the ecosystem be legitimate?

Jenny: Since we discussed this question earlier, I have been thinking about it. It reaches into question of a philosophical and religious nature. What is the purpose of nature? And if you consider man and woman, of course, as part of nature, then the question is, "What is the purpose of humans on this earth?" I would think that only a religious viewpoint could give an answer to that question.

Yes, I would say that it is good to pose the question about the purpose of an ecosystem because it challenges our thinking and makes us look around. Hilgard once said in a letter that the purpose of life is to develop good relations among human beings, which is interesting, because in many ways he was a religious man. Schweitzer preached an attitude of reverence for life--for all kinds of life, not only humans. But I have not thought these questions through sufficiently to come up with an answer. My vague notion is that I don't know what nature and life are attempting to do or what their purpose is.

Stuart: When you talk about the soil and describe soil as an independent natural body, what does that mean? What is soil independent from?

Jenny: When I cite the phrase, "soil is an independent natural body," I try to indicate that it isn't exactly my own belief. Of course, I believe that soil is a natural body. But if independence means that its body is independent from the environment then I cannot agree. But I will agree if the word "independent" means that we should view soil independently of other bodies, in other words, give it real status as a body not in contrast but in addition to other bodies like living organisms and rocks and mountains and lakes and swamps and whatnot.

Stuart: Sometimes I read statements like, "We want to study soil for what it really is." Do you think that kind of statement has any meaning? In a way, it suggests that soil is being interviewed, that soil is talking and soil is describing what it is all about. But couldn't one argue that soil is really being studied from the perspective of people who are looking at it so soil never really has a chance to speak?

Jenny: I presume when the statement is made that the person has in mind to look at soil and study soil regardless of its use for agriculture, forestry or other purposes. That approach is sometimes called the scientific study of soil in contrast to the practical study.

But I can't see how we can look at soil as a pure object--as a neutral object of study. I feel that our emotions and sense impressions and how we receive the sense impressions are involved in the way we look at soil. Sometimes I like to look at soil with the eyes of an animal like a gopher, a mole or an insect and think about how they view a soil. And then there are all the small animals in the soil that are blind because they live in eternal darkness and thus eyes have no purpose so they wouldn't look at the soil. They might listen to the soil or concentrate on smelling the soil to make contact with their soil environment. I think one might want to run experiments on how mice or small mammals or snakes react to soil. Whether they smell it or they perceive openings, structures--what their sense are keyed to--what responses they recognize.

Preventing the Long-Term Effects of Soil Erosion

Stuart: I would like to talk some more about soil degradation. I have interviewed some soil scientists who have said that they feel soil erosion is not a problem. Do you think soil erosion is a problem in this country?

Jenny: Soil erosion is a problem for many reasons. Certainly soil erosion leaves soil material where it may not be wanted. It fills up reservoirs, lakes, and ponds, so for certain people that part of erosion is not welcome. If we say soil erosion is not important, say, for agriculture, the answer is more complex. Are we thinking of producing enough food to feed the people of the United States? For that, I would say, soil erosion is not a prime consideration, if we have only a short term outlook, for example, for the next year or the next couple of years. But if we look ahead for a thousand years and longer, then, as I see it, erosion cannot be declared as insignificant.

But what we are really concerned about are the best soils and, with the present rise in population, erosion is an important factor for the future of our nation. Some economists say that we shouldn't bother about erosion, presumably because we have huge or very extensive areas of soils--of good soils, so we needn't worry about it and we have this problem of agricultural surpluses. Again, that points to a view that we need not be concerned about

Jenny: the soil. But if export of food is a vital component of our national economy, then even today we must be concerned about erosion.

Stuart: Are you personally concerned about erosion for other than these reasons or of this country perhaps not being able to meet its future food supply needs or about waterways being filled up with soil?

Jenny: I have a general interest in the American landscape because its immensity, diversity and beauty impressed me when I first came to the United States. I have the feeling that whatever change we impose on our landscape--the landscape becomes less beautiful than it was before--but I know, of course, that landscape architects would not share that view. They think they improve on the beauty of nature. But what interests me about landscape architects is that the way they want to improve on California's nature is to make it more similar to the European landscape. I suspect that in the training of landscape architects that their contact with California nature is not emphasized.

If you look at this campus, landscaping is not in the light of the native California vegetation. It is watered and plants are imported from Europe. It is different in being a new creation. I don't want to say that I consider aspects of the new landscape or of the un-natural, non-native landscape as unpleasant. It has many aesthetically attractive features. But I would like to see a landscaping project that stresses native landscape features. A year or so ago I was watching land development action in the Silicon Valley on the peninsula in Santa Clara County where the landscape people had huge machinery leveling hills or land undulations and moving the soil to make other little hills and depressions--a complete destruction of the natural setting. Now I have forgotten what we had started out with.

Stuart: I asked if you personally had other reasons for objecting to soil erosion besides the possibility that sometime in the future there might not be enough soil to grow crops and because of the filling up of waterways?

Jenny: I feel that certain soils should be preserved, and I consider soil in the same class as rare and endangered plants and animals. If we have justification to preserve and save them I would put soils in the same category. We should have places where in the future one can show people things like a natural museum and show them what a virgin soil looks like.

Hans Jenny Reviews His Contributions to the Field of Soil Science: State Factors Analysis, Contact Theory, Preservation of Natural Areas

Stuart: I had asked you in a former interview to describe people like F.H. King, Bradfield, and Truog. What contributions would you say Hans Jenny has made to the field of soil science?

Jenny: As I get older and live long enough most of my former enemies, also friends, have gone and I'm getting all sorts of recognition and awards for my contributions. The contributions are spelled out in documents; specifically, in the honorary degrees, and what is mentioned predominantly is my contributions to the relationship of soils to climate, particularly the nitrogen and carbon function related to the state factors. Some people stress my development of the state factors analysis, but I'm not always clear whether the people who give these awards fully understand what I was doing, although presumably the person who nominated me had a good understanding.

Sometimes I think about what I consider to be my contribution. I like to think about several things that I did but foremost I think of two areas or maybe three. One is the approach of state factor analysis which helped me in my own thinking on how to look at soils and how to comprehend the origin and distribution of soils. That approach helped me in viewing native ecosystems and I find I can follow discussions of soil scientists, ecologists and others very comfortably. It even goes further than the description of natural phenomenon. I get a view of history, specifically a history of the United States in terms of the state factors. In fact, a well-known agricultural historian--James Malin--used the factor approach in his description of the Great Plains area, saying, for example, that the introduction of the railroad did not originate within the area of the Great Plains, it originated from the outside of that great ecosystem and therefore played the role of the change in a state factor.

A second achievement that I would like to discuss is called the contact theory. Currently, this is a topic that is not being spoken of. You don't hear about it. You don't see anything in print. But I think it is an attractive theory with many interesting consequences. I don't know whether I'm disappointed or not that it has reached this stage. I can't help but feel that eventually this theory under a different name will surface again because I consider the reaction of solid ionized particles with each other is a cardinal reaction going on in nature. But presently the thinking of my colleagues in soil science and plant nutrition is oriented in a different direction, partly because, in their period as students, their colloid chemical background and training does not have a high priority.

Jenny: To continue, some of the things I did with the help of others are not known. There is not much of a record; it has not been spelled out. An example is the application of gas ammonia in fertilizer practice. That idea originated in our laboratory and was translated into practice by the Shell Chemical Company.

A third larger contribution, aside from the state factor and the contact theory, is the preservation of natural areas which I emphasized after I retired, and I like to think that this is my greatest contribution to soil science--the preservation of natural areas giving an opportunity to future generations of soil scientists to see what nature did with soils prior to human disturbances. That preservation is not, however, absolute. In case of a nuclear war many natural soils would be profoundly altered.

Stuart: How did you arrive at the idea of the state factors?

Jenny: I like to think that I arrived at the idea of the state factors independently of other people. But I'm not sure I can make that claim. I know other people, especially Hilgard and Dokuchaev in Russia, thought in terms of state factors, although they did not advance a scheme that enabled people to solve a state factor equation. I did state factor analysis as a graduate student. But I did not know that was what I was doing. The idea did not exist then. But I was thinking of--I did draw curves in a speculative way--how soil properties would vary with a change in their mean annual precipitation.

When I came to Missouri I started correlating soil nitrogen and carbon with rainfall and temperature and obtained statistically significant curves or correlations. And as I pursued it I realized, or observed, that the parent material of the soil was a factor, and lately that the biota were a factor. In one study I found that time was a factor. So I put these together in a formal equation, the clorpt formula, and then I noticed to my astonishment that I had discovered "the soil forming factors." And, in fact, later on I found that Dokuchaev in Russia already had set up such an overall equation. But the way he formulated it and defined the individual factors, his equation could not be solved. In my work it was solved because I defined more rigorously and differently the meaning of the state factors.

When I came to Berkeley, I organized the factor idea into the book called Factors of Soil Formation. At first it was turned down by the publisher and after it appeared the response of my colleagues was neutral or negative, but over the years it has achieved a secure place in the minds of many.

Hans Jenny on Mount Kiliminjaro, March 1981.

Stuart: Time, topography, parent material, climate and biota seem today to be so clear, so easy to see as factors that are responsible for the formation of soil. Can you describe what people thought about soil prior to the publication of your book? What did people think was responsible for the formation of soil?

Jenny: In the United States, as I recall it, little emphasis was placed on what we call soil genesis. The big issue was to describe soils, study their properties and record their occurrence. That was an overwhelming task that confronted the soils people and, though they may have thought about how soils are formed, little was published about it other than as a local issue because the job was seen as helping the agricultural growth of the nation. And I think the soil forming factor book helped to shift the view of the soils people in other directions, and it initiated soil courses at universities. But I could not estimate how significant the contribution was. Maybe other trends helped to put soil into focus as a natural body to be studied.

Pinpointing Important Issues

Stuart: How would you describe or how would you identify the forces at work that made an issue or make an issue an important one for the field of soil science?

Jenny: I like to think that the importance of a field of study has to do with theory. Others might say right away that importance lies with practical questions. With the case of soils, what can be done with the soil, or how much money can be made by doing things with the soil. But these are aspects that decide, you might say, the real estate value of soil. The significance of the soil in the view of other scientists--chemists, and physicists, biologists, geochemists depends on the kinds of theories that soil scientists come up with.

I think as a group we have been too hesitant in coming up with theories and speculations about soils. There is the fear that if you come up with an idea, and it is shot down, that both you and the field are hurt. In a close sister science--ecology--they encourage conceptual discussions much more openly than we do in soil science. In fact I believe there is no section in the Journal of the Soil Science Society that would--that could--receive conceptual discussion about soils unless they are specific theories in one of the acknowledged specialized fields, such as how to measure activities in soil solutions, that is, problems which are tied to the established network studying the reductionist properties of soil.

Current Theoretical and Practical Conflicts

[Interview 8: September 28, 1983]##

Stuart: What do you consider to be the current theoretical conflicts in soil science today and what would be significant about these conflicts?

Soil Erosion

Jenny: As to both theoretical and practical conflicts, the one that I see as foremost has to do with the tolerance value of soil erosion. A value of one and one-half tons of erosion per acre is considered acceptable, and the idea behind this is that natural soil genesis would balance that kind of loss each year, hence the loss is viewed as equal to the rate of soil formation. It is nowhere stated, as far as I can see, how this value was obtained. Did they analyze organic matter, or clay formation, or leaching or what? And, apparently, concern exists all over the United States. In fact, the Soil Science Society of America published a little booklet discussing various aspects of the soil tolerance value. My concern is not with the practical application but with the theoretical value of that amount because to know the rate of soil formation is of great scientific interest.

Climatic Change

Jenny: Another topic involving conflicts has to do with climatic change. Botanists and zoologists are very strong on climatic change, and many geologists and pedologists have accepted the thinking about climatic change. But how climatic change affects soil and in which direction--making it better or worse--is very difficult to determine. So, I would say, the effect of climatic change on soil genesis and soil stability needs investigation. From my climatic studies, I conclude that a rather substantial change is needed to be able to detect it in soil profiles, unless it is a very superficial impact, say in the first few inches of soil.

The Significance of the Soil Solution

Jenny: Another aspect of theoretical conflict--conflict may not be the proper word, it may be simply a difference of opinion--has to do with the significance of the soil solution. At present soil chemists seem to think that the soil solution is the beginning and the end of processes occurring in the soil, especially in relation to plant nutrition. There are, however, people, including myself, who wonder about what happens as the root pushes its way through the soil. It must push away soil mineral particles, humus particles, microbes, and I like to think that there might be an interaction of the plant root surfaces and the soil particles in addition to the soil solution effect.

The Boundary of Root and Soil

Jenny: The definition of the upper boundary of the soil is highly controversial. It is easy to recognize it in a plowed field and it is not too difficult to establish it in a forest, between two trees where pedologists consider the forest floor part of the soil, whereas some biologists may not. But the boundary under the tree is more complex; it is not measured and it is not discussed in the soil literature. Naturally botanists consider the roots of the tree as part of the tree but I find it more convenient to consider the root system part of the soil, which means that somehow the description of the root system and the analysis of the root system should be included in the soil description.

The reason I look at it this way is because of the difficulty in setting the boundary of the soil. If the root surface is the boundary to soil environment then the soil boundary is exceedingly complicated; it would extend to the finest root hairs and cell wall protrusions. It would be impossible to separate or to identify the soil surface. The experience with the electron microscopic view of the root-soil boundary teaches me that the boundary is not sharp. The boundary between the plant, the cell wall, the root hairs, and the mineral particles are difficult to separate and if we call a cell wall a non-living part of the plant then it is also difficult to decide where the plant ends and the soil begins. Those are questions and problems which have not been formulated or discussed so I would consider them to be concepts that are on the back burner.

I mentioned the soil solution as a cardinal concept in soil science. I would like to include the boundary of root and soil, specifically the contact effect, as a research area or field that does not exist in other sciences. True, in a sandy soil that

Jenny: boundary might not need to be considered because the extracted soil solution dominates the nutritional phenomena, but in clay soils the root/soil boundary is a crucial, unique area of soil science, botany and microbiology. There are many unsolved problems in soil science but they have not been articulated as unsolved theories or concepts.

The Isolation of Scientists: The Need for a Broad Outlook on Soils

Jenny: As a further point that I consider as a conflict or a shortcoming of soil science, I feel that soil scientists have a self-imposed isolation--an isolation from sister sciences such as ecology and some of the geo-sciences. By this I mean we are not discussing how our soil findings impinge on their thinking and factual knowledge and I believe that by not doing that we are doing a disservice to the excellent work that is being done in soil science.

Stuart: Why talk about the beauty of soil? Should this be an important aspect of soil science? Why or why not?

Jenny: I think you should have a broad outlook on soils and when we describe profile features in the field we should not restrict ourselves to the purely technical attributes. As we dig a soil pit all sorts of sense impressions occur to us about feeling--how we feel soil with touching, what we expect to see, such as color changes, mottling features involving color. I think that we should be aware of these and even articulate them, although at present that is not encouraged by the cultural upbringing that we have, and, specifically, the notion of soil as "dirt" that is embedded in our society.

Stuart: On that point, why should we be aware of these various properties that the soil has and articulate them?

Jenny: I think it provides us with a more personal relationship to the soil body and enlarges the notions and feelings about soil. I would say it makes us broader human beings by not restricting everything to purely technical features when we look at soil or talk about it.

Stuart: What would you say the ideal personal relationship with the soil should be?

Jenny: I don't know whether I can say what it should be. What I'm trying to say is that there are some personal relationships but they are repressed and we should become more open minded and accept these

Jenny: relationships. I can't help thinking of little kids making mud pies and enjoying it, getting a good feeling, and I think we still have the same feelings when we make a texture test with our fingers, but we would never admit that there is a connection to our childhood.

Stuart: How would you say society would benefit from people being more aware of soil and having a personal relationship with soil?

Jenny: If enough people came out in the open, so to speak, about their relationship with soil it would have an effect on society and I believe that society would take a more positive view of soil resources.

Stuart: How important do you think soil science and soil scientists are in influencing the rest of society's ideas about soil?

Jenny: I would say the effect is very small. I don't know exactly why. Some newspapers write articles about soils now and then. For example, some months ago the Christian Science Monitor had an extensive discussion about the deterioration of our soils but I can't detect any impact from these articles on the public's reaction, but maybe I'm not reading the right papers or don't have enough contact. But I see nothing, say, in letters to the editor, or in local news; soil articles, soil language is not offered. I don't know the reason for it.

Stuart: What suggestions would you make to people who are interested in bringing about a greater consciousness on the part of the wider population about soils--about the beauty of soil?

Jenny: The beauty is only one aspect. I think we could start with interesting artists about soils or interesting art critics about soils, and when paintings are shown in galleries--private and public--soils people, soil scientists, conservationists should become aware of it and should mention it at least in letters to the editor. Now I'm very much interested in this great experiment that David Bellamy is conducting with BBC. The story for his documentary, called "Bellamy's New World," is a botanical description of America with unusual emphasis on the role of soil and the evolution and development of natural America and people. I would be interested in seeing whether there is any response to his story which is very well written and consists of very beautiful photography.

More on Taxonomy: Analysing the Meaning of Sharp Boundaries

Stuart: You said earlier that Soil Taxonomy is based on discontinuity, separate soil individuals and thus the system was not logically balanced. Would you explain this in more detail? What is the true nature of soils and why does Soil Taxonomy not capture it?

Jenny: There are two aspects of soil classification that might be included in the first question which dealt with theoretical conflicts. I would say that the two important aspects are that soils vary from place to place, both abruptly and continuously. That means there are broad transitions. The second crucial aspect is the division between independent properties and dependent properties.

Ideally soil classification should be based on independent soil properties so as to avoid circular reasoning. For example, soils along a slope vary continuously. That means that if we start from the top and walk down the slope, the soil changes gradually, imperceptibly from the upper part of the slope to the lower part. Yet in soil surveys we draw a boundary and to a considerable extent that boundary is quite arbitrary and two surveyors may not agree where to draw it. Discontinuity of soil properties over a landscape are often associated with discontinuities of rock, and under such conditions where, for example, a sedimentary rock is adjacent to a serpentine rock, you get two different soils sharply set apart.

All we can say really is that the transition of one to the other soil occurs over a short distance. After all, animals like gophers and ants may move soil back and forth over a few inches or a few feet. And while the rock boundary may be sharp, the soil boundary may not be clear. But in terms of soil mapping, even if we draw a pencil line indicating where the boundary is, the thickness of the pencil line on the map may be ten or one hundred feet in the field and the transition zone would be drowned in that cartographic execution.

Also there are problems mapping the transition area or, as I mentioned, transitions on the slope which are very diffuse. Cartographers, as far as I know, have not developed good techniques for depicting continuous features between the bodies being mapped. So that may be one reason why the soil maps embrace a mosaic pattern of one body sharply separated from another.

Stuart: I guess that almost everyone would agree that soils differ from one place to another. There are many differences in soils. Do you feel that it is possible that there may not be any way to explain the pattern of difference in soils? That it might not be possible to show a pattern in the discontinuity?

Jenny: I guess I'm not sure that I follow your question.

Stuart: Soil classification is interested in describing the differences in soils from one place to another and it comes up with a system for doing that. But that is really asking the question: What is the nature of variation or what explains the difference in soils from one place to another?

I'm thinking, I guess, of an experience I had when I was at Missouri. I was taking a course from C.L. Scrivner in soil taxonomy and he took us out to a field that was fairly flat near Columbia. It was on one of their research farms. Bob Grossman was at Missouri then and he was interested in looking at a small area and in explaining rather dramatic differences from one area to another. So Scrivner took us out to this field that was maybe twenty acres or so and Grossman was there mapping soils. There were two pits that were close together and they were of different soil orders. And I don't remember for certain but I think that there might have even been a third soil order in that field which was fairly flat.

Now, if we are interested in getting at the fundamental nature of why soils vary from one point to another, how do we go about it? Or what is the nature of that discontinuity? It is easy to see it in some situations where there are dramatic differences in bedrock or climate but in other situations it doesn't seem quite that easy.

Jenny: The standard answer would be to look at soils from the viewpoint of the state factor equation. And if you have a given area, and simply draw a straight line, along that line the state factors vary discontinuously, which means they have sharp boundaries and that the soils would have sharp boundaries. If it is a state factor like topography or climate that varies continuously, grading gradually from one area to the other, from one magnitude to another, then the soils would show continuity. And so actually in field work that is what you do. You think in terms and when you have a marked change in slope you know you expect a change in soils. Or if you have a sharp change in native vegetation, you know you can expect a sharp change in soils or a sharp change in ground water conditions. These factors would produce marked changes. So that is what actually is used in soil mapping.

The problem is when you have a level area then you have to do a lot of augering to detect the soil differences and also the state factors differences and it may mean quite a bit of physical work to sort out what may be the cause. In the level areas such as on an alluvial plain there will be a variation in parent material from sand deposits to clay deposits or at times very sharp delineations that would create sharp soil boundaries, especially if combined with ground water fluctuations.

Jenny: In the book on the soil resource I have a diagram showing twenty profiles taken ten feet apart. And although they all belong to the same soil series they differ sharply from each other. And one may wonder which one should be picked as the typical profile because taking the average wouldn't have much meaning.

The Soil Science Society of America

Stuart: How does the Soil Science Society of America (SSSA) choose its officers? Would you please describe your experiences as president of that organization and how it was that you became president?

Jenny: My recollection is that the officers advance from the bottom of the ladder. You proceed through the ranks, from committee meetings, section secretary, section vice-chairman, section chairman and you go higher up the ladder. I think the process is very democratic. The one problem is to find enough people who are willing to start at the bottom and take on the assignments. Overall the society members know that the welfare of the society depends on its officers and many scientists are willing to serve and contribute to the welfare of the society. I'm not aware of there being much infighting going on. I feel there is an overall spirit of cooperation.

The goals of the society move along a very narrow path. But you asked me about officers and not about the goals of the society.

Stuart: Would you describe your own experience as president of the society?

Jenny: I was president a long time ago. It was hard work and one thing I did was to practically memorize Robert's Rules of Order to be able to conduct a meeting and handle any question that might come up. At that time there were still ill feelings among members of the Soil Science Society and the Agronomy Society, and I remember I had to take stands and become involved in the discussions. It was, I remember now, not a very positive experience although my colleagues at Berkeley were pleased that I had been elected and served as president.

Stuart: Why was it not a very positive experience?

Jenny: I think the society was involved in arguments with the Soil Conservation Service. And they were procedural arguments. Also, I remember we had a special guest lecture by a Professor Pendleton from somewhere in the tropics, Malaya perhaps, who proposed that in the tropics in some of these laterite regions, where the surface soils are poor, soil erosion was a beneficial thing. And that caused some heated discussions and I had to be an arbitrator in

Jenny: these discussions. I guess my action was not outstanding because I really had no experience or background in local politics or in political manipulations of societies. So I was worried about making mistakes or causing trouble for the society. I had never been a member of the manipulating group or manipulating leaders like Truog and earlier officers like M.F. Miller.

Stuart: How would you describe the politics of the society or the goals of these manipulating groups; what was it that they wanted, and how did they go about getting it?

Jenny: Being in California is a great disadvantage if you want to play a role at the national level. There are two big centers, one on the East Coast with perhaps Washington as the center. There are a lot of people there belonging to the Soil Science Society who have strong opinions. Then you have another center in the Middle West where people meet and communicate in all sorts of submeetings and discuss policies.

For Californians it was just too expensive and in the old days too time consuming to travel by train from Berkeley to New Orleans, or to Chicago, or to Washington, D.C. We were at the fringes and had to rely on a handful of people who knew us and that we knew from committee experiences or field work. The overall policies and aims of the society were in the hands of these two groups and perhaps a few outsiders who were there. Certainly the idea of having an office in Madison was decided by the mid-western group. I could see, for example, that people living in Alaska or Hawaii were especially isolated from participation in the day-to-day functions of the society. I think that presently the society is functioning smoothly. It produces interesting programs. But to me the society is too much confined to technical aspects and narrow agricultural aspects. But it is easy for me to criticize because I'm not doing anything to influence the society.

Hans Jenny's Personal Philosophy Toward Soil as a Living Body

Stuart: Earlier you said that you wouldn't wantonly destroy a natural soil. Why not? Does the soil have any right to be protected for any reason other than for what is best for humans? For example, if gold is important to our modern society why not dig for it even if in the process we kill the soil? Should we say what criteria should be used to deal with soil?

Jenny: This is difficult to answer and I have certain ideas about it but I wouldn't know how to justify them or to explain them. I could say that in my earlier years or especially as a child I did not have

Jenny: the ideas about soil that I have now. And I have not tried to trace how I came to have them. Overall, I believe in a certain reverence for life. For example, I would not hunt for pleasure. I would kill animals for food to survive or to protect myself. But that is different from killing life as a sport. Soil is a living body and so I include it in my overall reverence for life and for nature. But I don't think I would lie down in front of a bulldozer to try and stop the bulldozing of soil.

Somehow society has to reach a balance with respect to soil that is needed and soil that should be preserved. I think that I mentioned the policies of the landscape architects. I would like them to include in their planning the preservation of native landscapes rather than to bulldoze the landscape to pieces in order to imitate a European garden like at Versailles. When I was a student in Paris I was greatly impressed with the gardens of Versailles, but I'm not impressed anymore.

Stuart: On that point, why aren't you still impressed by the gardens of Versailles?

Jenny: It meant extensive destruction of natural vegetation and soil and it seems to me that we might have a reorientation of what we consider beautiful and attractive in nature. But I realize the bottom line is the survival of human beings. So what I'm advocating is how the survival is to be accomplished in a way that is most nature-preserving. I believe that the proper word would be to be a steward rather than a shortsighted exploiter.

Stuart: Earlier you said that cultivating the soil as a gardener using a spade, or a farmer using a plow would be a positive action. Why would that be positive?

Jenny: I might look at a cultivated soil as I might look at domestic animals which are no longer pure nature. But as humans we have a relationship to it, we want to gain something from the animal or the soil. We don't want to destroy it. So as to gardening and plowing, we should consider what would happen if these activities were stopped, whether the soil would not be too far removed from its virgin state and could revert to it. I like to feel about the soil that I cultivate in a garden or on my farm the same as if I have a relationship with a dog that serves me and I serve it.

Society usually assumes that man is the highest product of nature and that nature should serve man and that to change nature is laudable, but I'm not sure that is my view now. I feel that I'm part of nature and have a responsibility to nature. And society considers these aspects too in some ways. Take Central Park in New York, or Golden Gate Park in San Francisco. These cities want to preserve these areas. They don't want them destroyed, for all

Jenny: sorts of reasons including aesthetic and health reasons. So I would like to see that attitude broadened from local specific parks to the way we view national and state parks.

Stuart: Several times in referring to the soil you have referred to it as a substrate. Why?

Jenny: Substrate is a term used a lot by ecologists, especially botanists. And it means the foothold of plants. It could easily be a rock or a soil--the conventional view of soil--or a swamp or a pond. So it is probably broader than soil. But when I use the word substrate I really don't try to make a fundamental distinction between soil and substrate. I just use it in ecological thinking, so I wouldn't want to make an issue of it.

[Interview 9: September 30, 1983]##

Stuart: What were you thinking when you said that you would like to think that good soils make good people but that this definitely is not tenable.

Jenny: We know that soils affect the health of plants and animals and by analogy we could conclude that soils could affect humans, provided human nutrition is restricted to the products of certain soil areas. We could also extend this and talk about a healthy mind in a healthy body and that would bring us closer to the question of good or bad and ethics. But I'm not aware that goodness or badness is related to nutrition at all, although a certain school of thinking views human activity with an environmental impact as having a great deal to do with the state of our health, aside from genetic patterns. So I think it could be possible that there is a link, but I'm not aware that any documentation exists or has been attempted. Maybe not only through nutrition but also by dealing with the soil physically, as a gardener or a cultivator, may lead to a broader attitude about life--to becoming a steward or a husband of the soil resource--and that might psychologically develop an attitude that is good for the human individual and for society.

Stuart: Earlier you discussed Wiegner and Bradfield and the way they looked at soil and then you said at one point that you see soil a third way. How have you looked at soil in a way that differs from Wiegner and Bradfield?

Jenny: I thought we had discussed that.

Stuart: I think you described how Wiegner and Bradfield looked at the soil--what their concept of the soil was. But I wasn't sure where you placed yourself.

Jenny: Perhaps I'm not sure about Bradfield but I think I'm placing more emphasis on the soil as a living body and that this gives me a different relationship with the soil.

As to this idea of reverence for the soil--I don't think they ever thought in such terms. I think Bradfield thought more in terms of practical applications of soil and for Wiegner, as I mentioned, soil was an opportunity to apply and expand colloid chemistry. Also I don't think they had the idea of soil as a natural body which became prominent beginning in the twenties and the thirties.

Stuart: At what point would you say your own feelings about reverence for the soil and valuing the soil as a living body became evident or clearly defined and what factors explain it?

Jenny: I became conscious of it during my Berkeley period. Before that I was so busy collecting data, putting curves together and dealing with soil in a material way. At Berkeley, with my teaching, stimulating colleagues and students, I began to think more about the soil and to see it in a broader context so gradually I evolved a clearer picture. I think too, in California, the experience of getting into areas that are or were essentially untouched by man had something to do with it. In Missouri I don't think I had many contacts with natural soil systems or ecosystems.

There was no hiking; trails had not been developed. The environmental approach was not yet in the picture. I remember that when I used to go out--including when we had snow and I would try to go skiing--that there were always fences, so there was really nothing to go for. The popular sports were indoor sports, or football and baseball and related sports but not hiking and enjoying nature. In the winter time it was too cold and in the summer time it was too hot and humid. I went on a few trips with botanists. Their main theme was identifying plants rather than looking for a relationship with nature.

Stuart: Many chemists who work with soils are content to stay in the laboratory and take up very narrow detailed studies of the soil. Why didn't you do the same?

Jenny: I would say that it depends on the background. Working in the fields with horses and cattle, walking behind the plow, trying to make a straight furrow--these activities always were in my background. And in addition, my experiences on my great soil excursion through the United States at an age when I was very receptive to new things sharpened my approach of looking outside the window and thinking about the soil outside.

Searching Out Scientific Truth

Stuart: At one point you used the phrase "scientific truth." That's a phrase I have heard a number of soil scientists use. What would you identify as being the object of the quest for scientific truth in soil science and would you say there is any conflict between what it has been, what it is, and with what it ought to be?

Jenny: In more general terms, I believe the phrase "scientific truth" is used for propaganda purposes. I'm actually inclined to think that it is predominantly an American term and it is used in controversies or in opposition to such philosophies or views as are found in Christian Science, or as are used in arguments put forth by public relations people. I use that expression but when I do research I never think of it that way. I like to know what is happening, what the facts look like in terms of a frame of reference, in terms of concepts, equations, laws, and ideas. But the word "truth" is not foremost in my mind when I think about it. When trying to develop a definition of soil, the task really has no more to do with truth than following the rules of chess playing is related to truth.

The Essence of Soil Science

Stuart: What would you describe as being the area of interest that soil science has been most concerned about? How would you classify the different investigations that have gone on in soil science? And do you think you could put them all into one category? Would you describe these investigations in a general way as having some principal interest? What is the essence of soil science as it has been conducted?

Jenny: It would be interesting in this light: to look at the abstract collection of the papers that are presented at the national meetings of the SSSA. This is a substantial book and may contain hundreds of abstracts of topics that are presented. It would be quite interesting to digest that information and see whether you come up with a real thread that tells you something about soil science. In such a volume the soil science you and I are talking about is probably represented thinly because the soil science society, in order to maintain a substantial membership, admits a variety of interests. In fact, I believe, anybody who has any interest in soils is welcome to join the society and present papers. So if we look through the collection of abstracts, we get papers from personnel from the navy, from the army engineers, from

Jenny: an enormously broad spectrum of the manifestations of soils, and what you and I call soil science is represented, I would think, by a minority of papers.

Now let's see. What did you ask me specifically?

Stuart: What would you say the essence of soil science is as it has been conducted?

Jenny: I would say the essence is the production of food and fiber. That is the essence of the people who are in the Soil Science Society of America. There are other people, many more of them, namely engineers and engineer geologists who deal with, study, and write about soils in an entirely different vein. Their concept is what is known as soil mechanics. This covers the engineering processes of the soil. And such ideas as "ecosystems" and "natural body" simply play no role. Does that come closer?

The Value of Cooperation Between Soil Scientists and Social Scientists

Stuart: In talking to some people about questions concerning the social consequences of soil science knowledge, I have been told, "That is something that the social scientists should be concerned about." And then I ask people where they draw the line between the concerns of soil science and the concerns of social science. How would you draw the line; where would you draw the line?

Jenny: Looking at my own case, the boundary is set by my training and knowledge. If I had good training and an understanding of social science, I might push the field of soil science considerably into the field of social science, and I would think that a social scientist cannot go further into the field of soil science than his knowledge permits him to go. Of course, you could say that you can learn and broaden yourself. But I think the best solution would be that the two scientists combine, so a fruitful enterprise would be developed through collaboration of a pedologist and a social scientist. But even so the pedologist should have a real interest in social science questions and vice versa.

I think cooperation has actually taken place most often between soils people and anthropologists. I remember that many years ago I worked with [Robert] Heizer, who passed away. He was a professor of anthropology and an authority on American Indians. We did field work together. Actually what he wanted to know was whether soils information could furnish a method of dating for his Indian burial finds. I was interested in whether, from his

Jenny: burials, we could draw conclusions about the rate of soil profile formation. From these answers you can see that this was before Carbon 14 dating had been developed.

Stuart: Why should the question of the importance of soil to the evolutionary process be of interest to the soil scientist?

Jenny: Soil scientists get what you might call status support by knowing that soils are important in nutrition for crop plants and forests--that they are an integral part of support for the ecosystem. But if it could be shown that soils have the additional impact of evolutionary trends, specifically affecting the rate of mutation, it would broaden the role of soils in the fields that border soil science. I would like to believe that soil scientists would be interested in these perspectives. The time may be fruitful now with all the emphasis on genetic engineering so we might think more about these fundamental changes in organisms. The reason I like to think about these possibilities is the existence of many endemic plant species on particular soils--mostly extreme soils.

The Positive Aspects of American Soil Science

Stuart: What would you say the positive aspects of American soil science are?

Jenny: The accumulation of knowledge in books and journals is a positive contribution to society. I'm proud of it and see it as a positive thing. As a negative aspect, I'm not aware that laymen would read soil science literature or subscribe to journals because our accomplishments are rather confined to technical aspects and we have not stressed the conceptual problem existing in soil science--problems and expositions that would find their way into the popular media. The negative aspect is the fact that in view of the great importance of soils to all peoples on the earth, the number of soil scientists is ridiculously low. I would like to see more work done along the same lines we have been pursuing and more work on the concepts of soil as a natural body and a living system. This approach being a turning away from extreme reductionism.

Stuart: Why do you feel that the accumulation of soils knowledge in books and journals is a positive contribution to society?

Jenny: In plain words, it contains a wealth of information that many people may use. I'm thinking now of gardeners, farmers, and engineers--many members of our society. And since our society admires knowledge, it follows that soil science knowledge is a good thing. But there isn't enough of it. And that accumulated body of knowledge is also valuable to people who have conceptual interests

Jenny: in our earth. I myself have contributed a good number of scientific papers to that body of knowledge and I like to think that a portion of these papers have been valuable.

Stuart: Let me ask this last question. If you were building a model for an ideal soil science what would it be?

Jenny: I'm stunned by that question really and don't want to answer it glibly right now so let's postpone my response.

III THE SOIL SCIENCE PROGRAM AT THE UNIVERSITY OF CALIFORNIA AT BERKELEY

[Interview 10: October 10, 1983]##

Comparing Berkeley with the Concept of a Model Soils Department

Jenny: In response to a previous question, I would like to talk about a model of a soil science department at a university. That is the kind of experience that I have had. Another type of soil science department would be with an agency, like the Soil Conservation Service, which has a large staff that deals with huge problems of soil management. Speaking about the Department of Soils at this university, I had the experience of being the head of the Department of Soils for a number of years.

I would think that a model department should be relatively small, to be flexible, but as I look over the topics that should be discussed in the department, it really is not small. If I think of the fields I should like to see studied, I would think the department should have a geomorphologist, a soil physicist, a soil chemist, a teacher in pedology, one in microbiology, one in zoology, and one in plant nutrition. Most soils departments have a few of these fields represented. I would also like to include a person who would study the interaction of soil science with the social sciences, and maybe a person interested in the history of soil science.

I would add something we have never had here at Berkeley that other universities have--a few joint appointments with people in action agencies who do applied soils work on a large scale. They might give a course or two once in a while. This would, I hope, stimulate their activity and stimulate likewise the activity of the academic staff. So it would be a sort of mutual education.

Jenny: Now, in this lineup I mentioned, there is one problem for the people involved. We have had, for example, a case where one professor represented a field and he was unhappy because he was alone. He felt that the other faculty members were not sufficiently interested in what he was doing so, unfortunately, he left us and went to another university that specialized in his field and had several members in that section. This gave him an opportunity to discuss his research and teaching with colleagues in his own speciality. So when I said you should have one member in each branch, maybe that is not enough for all the branches. In the past, for example, we have had two or three people in soil chemistry and at least two people in soil physics. So that would be a problem: whether one should have fewer subject matter divisions and more people in each subject, or many fields of study with only one or two representatives each.

Now what I described is a sort of an idealized arrangement because the size of the department depends a great deal on the number of students who want to take courses in that department. You can't have more professors and staff members than you have students. The university could not operate on that sort of a scheme. So it would really depend, on one hand, on the demand by the students and, secondly, on the demand that agriculture or forestry makes in creating new knowledge for their problem areas.

We had another problem in the past. Twenty or thirty years ago we had the best curriculum nationwide in soil science in that it gave the students a thorough knowledge in the basic sciences and disciplines, like chemistry, physics, mathematics, biology, and languages and, of course, over all a thorough knowledge of soils. This is in contrast to other institutions which, for example, taught soil fertility on three levels: a beginning, intermediate, and advanced course. And in the latter, students heard for the first time about the law of mass action. Our students at that time had a good understanding of the law of mass action before they took any courses in soils, so we could fully educate this group of students.

But it turned out after they left us that they were over-educated. When it came to positions in industry or in the government, such as the Soil Conservation Service, their salaries were not higher than the salaries that came with, what you might call, a less competent education in soils. Moreover, our students tended to be more critical than was appreciated by the officers in the agencies. It came to the point where the jobs, say, in the Soil Conservation Service, were predominantly given to those students from the institutions that required the least knowledge in the fundamental sciences. Thus, to design an ideal soils department is not an easy task and would depend a great deal on the broader interests of the university as a whole and the state of job opportunities in the field of soil science.

Defining Pedology and Soil Science

Stuart: Would you give me your understanding of the difference between pedology and soil science?

Jenny: Among certain groups of soils people the expression "soil science" is broader than the term "pedology." Soil science would include all research activities in the field of soils: microbiology, chemistry, physics, and whatnot, whereas pedology, in the view of those people, refers to those soils activities that deal with field work, and lab work about soil genesis, about classification, about the soil as a dynamic body, about the profile of the soil, including soil behavior in the field. In other places, however, especially in Europe, I think pedology is used as an equivalent to soil science. And that is exemplified in the name of soils journals that contain the word "pedology" in their titles. I can think of Russia, also Belgium, and maybe others.

But even here in the United States, actually in our own department, people use the word pedology either in broad connotation or in the narrower connotation of soil genesis and classification. I prefer the word "pedology" because I don't feel happy with the expression "soil science." The word "soil science" bothers me in a connection with a young field like the study of soil. Also I prefer to be called a pedologist because it relates to the words botanist, geologist, and zoologist rather than the other such terms, as soil scientist, plant scientist, and animal scientist. But I may be alone with that attitude. In fact I have never discussed it with anybody. But I know Paul Gersper had his title changed to pedologist.

Stuart: Why did you say, "The word 'science' bothers me in connection with a young field like soils?"

Jenny: I think the knowledge we have about soils and the theoretical framework is not yet advanced enough to be labeled as a science, in comparison with physics, chemistry, botany, or geology.

Development of the U.C. Berkeley Department of Soils

Stuart: Could you describe in a general way the development of soil science here at Berkeley since the time you came here?

Jenny: Only during the time I have been here?

Stuart: Yes. Well, actually we shouldn't limit it to the time that you have been here.

Jenny: No. I'd like to begin earlier.

Stuart: Surely.

Jenny: And that would begin with Hilgard.

When this university opened, after the passage of the Land Grant Act, it hired a professor of agriculture. However, the appointment was not a success. I don't need to go into the details because it involves politics and he was either fired or he resigned. The regents invited Hilgard to give a series of lectures in 1874 and then appointed him professor of agriculture and professor of botany. He had already a national reputation as a geologist, soils man, and chemist. He had been elected to the National Academy before he came to Berkeley, and after he arrived he continued his soils work and pursued it until he retired, I think, in 1905 or 1907.

During that time he announced his great discovery of the effect of climate on soil formation and he received many honorary degrees from the United States and from Europe. After he retired, a new dean of the College of Agriculture, who came from an eastern university, appointed Professsor Shaw, whose background was in soil surveys. Hilgard had also had a laboratory for analyzing soils and fertilizers, as an advisory service to farmers. After Hilgard retired Professor Burd got involved with these activities.

When I came here, in 1936, we had a Department of Soil Technology, headed by Professor Shaw, and it included Professors Storie, Weir and also Professor Bodman, whose field included soil physics and soil genesis. I joined the Department of Plant Nutrition, headed by Professor Hoagland, and it contained Professor Burd as a soil chemist, and people in biochemistry, plant physiology, and x-ray analysis. That department had a good deal of interest in soils; mainly in the chemistry and biology of soils.

A few years after my arrival, Professor Kelley was transferred from Riverside to Berkeley, and a department of soil chemistry was established, but he was the only professor in that department. Shortly afterwards Professor Shaw suddenly died, maybe in 1939, and a reorganization was proposed. Professor Kelley and the remainder of Professor Shaw's department, and I, and my collaborator Overstreet, were joined together into a Department of Soils, with Kelley as the head of the department. Some years later, I think shortly before World War II, Kelley gave up the chairmanship and I took it over.

In the early 1950s Professor Hoagland died and his department was gradually dissolved--with people moving over to biochemistry; I had resigned my chairmanship, and the Department of Soils and the remaining people in Plant Nutrition were combined into a Department

Jenny: of Soils and Plant Nutrition. The dean, Dean Hutchison, was a sponsor of soil activities and he favored the erection of a soil science building that would have been located on the north side of the Hilgard Quadrangle. The money was made available and the plans had been drawn but then an intrigue developed. Rumor has it that Warren Schoonover, the extension soils specialist, engineered it. The idea being that this department should be moved to Davis and that the new building should also be transferred to Davis.

The upshot was that this building did move to Davis and is now known as Hoagland Hall and a good portion of the staff also moved to Davis and a portion stayed here. Also the chairmanship of the Berkeley campus was fused with the chairmanship at the Davis department and the chairman of both departments was located at Davis. In the long run that did not prove efficient and the scheme went back to having separate heads of departments at Davis and at Berkeley. These developments weakened the soils work at Berkeley because most of the soil survey people decided to make Davis the campus headquarters.

The Impact of Moving the Soils Work to Davis

Stuart: Let's go back to the movement of much of the soil activities to Davis from Berkeley. Could you elaborate some more on how that actually came about? Who was convinced that was best and how was it done?

Jenny: For many years the soils department at Davis and other departments at Davis were essentially an appendix to the Berkeley campus. For example, when I was head of the department here, I was at the same time head of the department at Davis, so I spent one or two days each week at Davis. Gradually the Davis campus became more prominent and I think the overall university plan envisioned that Davis would become a major campus in the state university system. There had always been political activity trying to get departments from Berkeley to Davis. Davis wanted to be the major agricultural campus in the state.

I remember, as head of the department, I attended many meetings--joint meetings--of department heads here and at Davis, and the Davis people made no bones about it that they wanted the Berkeley people to move to Davis. Large funds were appropriated for the Davis campus and they built it up on a magnificent scale. The soils department was enlarged with numerous new positions, and the positions were filled, and the remaining Berkeley group stayed and the pressure for the remaining people at Berkeley and the

Jenny: remaining departments at Berkeley ceased. We were left, so to speak, alone. But gradually the dominant soils work shifted to Davis.

If I say the dominant soils work, I would say certain phases of soils work, especially soil mapping and classification. But soil chemistry stayed at Berkeley prominently, with emphasis on thermodynamics and the work of the boundary zone of roots and soils, also work in soil physics and in soil genesis. After I retired, a number of people reached retirement age, or passed away, and the department was not encouraged to refill a number of these positions, and the department reached a low point.

This second draining of positions was caused, I'm told, by the fact that Berkeley biologists missed going on the bandwagon for genetic engineering, and biologists got together and initiated a push to expand the work in biology on the Berkeley campus. But because of the tight money situation the creation of new positions was difficult and so to add new position they "robbed" empty positions on the campus and especially in our department, where up to eight positions were unfilled. As a preliminary tactic to weaken the department, a committee was appointed to study the scientific accomplishments and reputation of the department during the last five or ten years. The resulting report indicated that the previous high reputation of the soils work at Berkeley had declined and that the reputation of the department had lost its glamour.

The Impact of Moves to Combine Biology with Soils

Jenny: This was used as an additional argument for taking over the vacant positions by the Department of Plant Pathology which was the home ground of a molecular biology group. I should insert an additional factor which complicated the situation. Many years ago--maybe twenty years ago--Professor Daniel Arnon was in the Department of Plant Nutrition and then joined the Department of Soils and Plant Nutrition, but he was not happy with the situation and he had enough reputation and prestige to leave the department and create a new department--the Department of Cell Physiology.

During the 1970s, Professor Arnon retired and the question came up as to what to do with his department which was relatively small. The administration decided to combine the Department of Cell Physiology with the Department of Soils and Plant Nutrition. But Arnon's group opposed this combination because they didn't like the name "Soils and Plant Nutrition." After much argumentation the two departments were fused and the new name was the Department of Plant and Soil Biology and Professor Eldor Paul, a soil micro-

Jenny: biologist, was called from Canada to become chairman of the department. I regretted the change in name because the word soil was degraded from a noun to an adjective.

Shortly afterward the people from former cell biology decided to pull out of the department and join the Department of Plant Pathology and they succeeded in convincing the administration that they could obtain or take over the unfilled positions of the former soil and plant nutrition people and also their equipment and laboratory space. That happened about one year ago and the Department of Plant and Soil Biology is now in the process of rearranging itself.

For a while some people thought that the department would be eliminated but now the future looks more promising because one of the empty positions is being filled by a young pedologist and three members of the department got a very substantial research grant from the National Science Foundation. So the people in the department are more optimistic now about their future.*

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*What I said at the time I was interviewed by Kevin Stuart in 1983 depended on the situation of the department, which has now changed. [1989. H.J.]

Transcriber: Kevin Stuart
Final Typist: Catherine Winter

TAPE GUIDE -- Hans Jenny
Interview with Kevin Stuart

Mr. Stuart used portions of a tape for several separate interview sessions on differing dates. The following guide is taken from his notes. The tapes deposited in the Microforms Division of The Bancroft Library are numbered based on these notes.

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*Interview number [ed.] one to three with Hans Jenny. These are typed notes of my interview. This interview was not recorded.

**This is a transcription of my written notes taken during the interview with Dr. Jenny. It is not a transcription of the taped interview. this interview is the first one taped.

PRESERVING NATURAL SOILS*

Hans Jenny

On Saturdays I used to conduct field trips with students to the Berkeley hills and parts of adjacent Contra Costa County. On some sites we collected soil samples to various depths and analysed them in the laboratory, which is time consuming and expensive. Returning to the same area a year later, some sites had been built upon, or bulldozed, and the instructive teaching objects were lost forever.

The idea that areas of scientifically important soils should be preserved has been voiced by several pedologists, but whenever I mentioned to government officials the plight of rare and endangered soil species I encountered smiles and hearty laughs.

Years ago, on one of Carl Sauer's stimulating field trips I mentioned that preserving crucial virgin soils might be my best contribution to soil science. After my retirement I started action in earnest. The new experience of dealing not with students of soils but with owners of soils, with developers, supervisors, and the hierarchy of government workers and officials was quite an eye opener and I realized that I had lived in an ivory tower. In fact, I was, and still am, too impatient to be a good politician. Had it not been for my wife, friends, and devoted well wishers today's valuable string of soil preserves would not be in existence.

I am not sure to what extent I am a nature lover. Yes, I admire the view of the Bay, I loved mountain climbing, and skiing in the Sierra, but on the farm in Mendocino County I am fighting a nature that wants to take back the farm land that my previous owners wrested from her. Nature, though admirable in many ways, is very cruel in the life struggles of living organisms.

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*Because it was not possible to interview Hans Jenny about his activities dealing with the preservation of natural soils, he agreed to write this brief essay detailing his experiences.

My soil preservation activities are science-motivated. We need natural, undisturbed soil benchmarks to gauge what soils are doing in the course of their evolution from sterile rock to supporters of life, and how farmers and foresters modify them. Today few, if any, of the once highly fertile American soils are left in their pristine state.

In the West, government lands that settlers didn't want ended up in the hands of the federal Bureau of Land Management (B.L.M.) that owns choice pieces of remarkable soils, ecosystems and landscapes. I have examined many B.L.M. maps and pinpointed suitable areas of striking combinations of soils and vegetation that would make marvelous nature museums for upcoming scientists and naturalists. Alas! I am finding myself in fierce competition with mining interests, hunting clubs, political-clout-wielding rifle clubs and off-road vehicle enthusiasts, war game groups, fishermen and loggers who all want these priceless bits of nature for their own purposes.

Biologists and environmentalists who succeed in preserving locales of rare and endangered plant and animal species, and who establish wilderness areas, simultaneously protect the soils that go with them, but these soils may not be the ones pedologists are anxious to maintain. In one instance, a site that soils people recommended for Natural National Landmark designation was opposed by a botanist because the soils did not harbor rare and endangered plant species. We need recognition of the value of entire ecosystems, and landscapes, whether or not they are viewed as beautiful and spectacular.

February 1989

Jean and Hans Jenny, 1983.

PRESERVATION OF NATURAL SOIL AREAS

Hans Jenny

Some virgin soils are agriculturally unattractive but they possess features of scientific soil interest that make them valuable as benchmarks of soil origin and evolution. These soils are indicators of how soils behave, and they function as natural museums. They deserve preservation.

The writer and his wife, aided by friends and well-wishers, and sympathetic officials, succeeded in setting aside in California the following virgin areas of soils and their native vegetation:

Mendocino ecological staircase (Jughandle State Reserve), a 1000 acre tract of geologically uplifted marine terraces that are several hundred thousand years old. The level terraces carry ancient soils and support rare types of plant and animal life.

Pygmy forest on the northern California coast. These soils, derived from beach sands, are highly acidic and they have developed white, 6-inch wide bands of fine sands, and foot-thick, concrete-like hardpans. They are covered by pine barrens and dwarfed cypresses. About 80 acres are part of the University of California Natural Reserve System, and an adjacent 40 acres belong to The Nature Conservancy.

Laterite soils, near Ione, California. A parcel of 40 acres, administered by the State, displays kaolinite soils and iron crusts many feet thick. These fossil soils are remnants of a tropical climatic period that reigned millions of years ago.

Red Mountain and Red Hills. These are BLM lands comprising red soils from the ultrabasic rocks, the former under high rainfall, the latter under low rainfall.

Mt. Shasta Mudflows. Catastrophic mud flows descended from ice-covered Mt. Shasta (14,000 feet) over a period of several thousand years. The deposits developed into an age-sequence of fertile forest soils.

March 1987

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Hans Jenny

1899	(February 7) Born in Basle, Switzerland. Raised in country and city.
1918-19	A year's work on farms.
1923	BS in agriculture, Federal Technical Institute, Zurich, Switzerland.
1927	Doctor's degree in agricultural chemistry (same place) with thesis in colloid chemistry.
1927	Rockefeller Foundation Fellow, working in soil microbiology with Professor S. A. Waksman (Nobel Laureate) in New Brunswick, N.J.
1928	Assistant Professor of Soils, University of Missouri, Columbia, MO.
1930	Received nitrogen research award from American Society of Agronomy.
1936	American citizenship.
1936	Associate Professor of Soil Chemistry and Morphology, University of California, Berkeley, CA.
1939	Published book on colloids.
1941	Published book on Factors of Soil Formation.
1943-49	Head, Department of Soils, Berkeley, CA.
1946	Guggenheim Fellowship for the study of tropical soils in Colombia, S.A.
1949	President, Soil Science Society America.
1954-55	Year's leave of absence to study soils in India, Egypt, Europe.
1957	Honorary doctor's degree, University of Giessen, Germany
1960	Published book on nitrogen and carbon in soils of India.
1961	Published book on E. W. Hilgard.
1963	(Two months) Visiting Professor of Forestry, University of Seattle, Washington.
1963-64	Fullbright Lecturer, University of Giessen, Germany.
1967	Retired.
1967	Honorary Doctor of Laws, University of California, Berkeley.

1967-69 Rehired for teaching.

1968 Member International Advisory Council on soil fertility to the Vatican in Rome.

1968-present With wife active in preservation of natural areas: Mt. Shasta mudflow research area, Pygmy forest ecological staircase (Jug Handle State Reserve), Apricum Hill laterite crust near Ione, Jepson Prairie Vernal Pools (Mima Mounds).

1971 United Nation collaborator on ecosystem research in Bulgaria.

1974-76 Member North Coast Advisory Committee to Board of Forestry; forest soils consultant to California Attorney General.

1975 Honorary member Soil Science Society America.

1977-present Collaborator Environmental Science Division Oak Ridge National Lab. Oak Ridge, Tennessee.

1970-80 Writing book on soils and ecosystems, field research with colleagues in pygmy forest area supported by N.S.F.; laboratory work; conducting field trips for faculty and students; giving seminars and lectures to local and national audiences; serving on thesis and promotion committees and Academic Senate committees; evaluating technical manuscripts of local and national authors; testifying at legislative sessions; consultant to Deputy Attorney Generals; examining BLM lands and writing proposals for their managements and preservation (Red Mountain, Red Hills, San Benito Mtn.); extensive interviews for Oral History for Bancroft Library; etc

1980 Published 377-page book: The Soil Resource, Origin and Behavior.

1981 Conducted soil sampling safari to Mt. Kilimanjaro, Africa.

Elected, with wife Jean, a Fellow of the California Native Plant Societ

1982 Received, with wife Jean, Certificate of Appreciation from The Nature Conservancy.

Collaborating with BBC (London) in filming Ecological Staircase in Mendocino County, a portion of "Bellamy's New World".

1983 B.B.C. dedicates the book "A Botanical History of America" to H.J.

Contributing author to editor Wes Jackson's book "To Meet the Expectations of the Land".

Honor Award, Soil Conservation Society of America.

1984 Honor Award, California Chapter, American Society of Agronomy

1984 (February) "The soil in perspective." Scientific symposium held on the the Berkeley campus in honor of Hans Jenny's 85th birthday. Speakers: Peter Birkeland (Univ. Colorado), Daniel Dindal (S.U.N.Y. Syracuse), Hans Jenny, W.Mac Post, Jerry Olson (both at Oak Ridge Nat. Lab), Mike Singer (Davis), Gary Sposito (Riverside), Paul Finke (Berkeley).

1984 (May) "My friend, the soil." Interview, Jenny Soil and Water Cons.

1985 Address at opening of Soil Conservation Exhibit of the Sierra Club, John Muir Historic Site, Martinez, CA

1986 Elected Honorary Member of the International Society of Soil Science.

1984-87 Member of the California Soil Conservation Advisory Committee, Sacramento.

1985- Research in pygmy forest and U.C. greenhouses.

1989 Awarded Berkeley Citation at reception celebrating Hans Jenny's 90th birthday, attended by some 200 colleagues, former and present students, and fellow conservationists. March 12.

Publications

Hans Jenny

1. H. Jenny. Ein Beitrag zur Kenntnis des Kalkgehaltes in Schweizerböden [A contribution to the knowledge of the calcium carbonate content of Swiss soils]. Schweiz. Landw. Zeitschr. "Die Grüne," 51: 57-61, 1923.

2. H. Jenny. Theoretische Grundlagen der Fütterungstechnik [Foundations of animal feeding. A popular article]. Schweiz. Landw. Zeitschr. "Die Grüne," 51: 1176-1180, 1923.

3. A. Frey (-Wyssling) und H. Jenny. Die Bedeutung der Wasserstoffionen-Konzentration (pH) in der Biologie [The significance of pH in biology]. Natur und Technik (Schweiz. Zeitsch. für Naturwissenschaften) 1924/25: 23-28, 1924/25.

4. H. Jenny. Bericht über Bodenuntersuchungen im schweizerischen Nationalpark [Report on soil investigations in the Swiss National Park]. Bericht der Kommission für die Wissenschaftliche Untersuchung des Nationalparks für das Jahr 1924, Zürich, 1925, 8-9.

5. H. Jenny. Wechselbeziehungen zwischen Dünger und Boden [Interactions between fertilizer and soil]. Schweiz. Landw. Zeitschr. "Die Grüne," 53: 1295-1298, 1925 (?)

6. H. Jenny. Das elektrolytische Moststerilisierungsverfahren, System Hans Jenny. [Electrolytic sterilization of cider, System H. Jenny]. In "Süsser Most im Fass das ganze Jahr" Verlag Landschäftler A.G. Liestal, Schweiz, p. 29-30, 1925 (?)

7. H. Jenny, Reaktionsstudien an schweizerishen Böden. Landwirtschaftliches Jahrbuch d. Schweiz 1925, 261-286.

8. H. Jenny, Die alpinen Böden. In: Braun-Blanquet, J. unter Mitwirkung von Jenny, H.: Vegetationsentwicklung und Bodenbildung in der alpinen Stufe der Zentralalpen. Denkschriften der Schweiz. Naturf. Ges. 63(2):295-340. 1926.

9. H. Jenny, Anleitung zum quantitativen agrikulturchemischen Praktikum. Von G. Wiegner unter Mitwirking von H. Jenny. Verlag Gebr. Bornträger, Berlin 1926, 348 S.

10. H. Jenny, Kationen-und Anionenaustausch an Permutitgrenzflächen. Kolloidchemische Beihefte 23: 428-472, 1927. Reprint as doctoral thesis.

11. G. Wiegner and H. Jenny, Ueber Basenaustausch an Permutiten. Kolloid-Zeitschrift 42:268-272. 1927.

12. G. Wiegner and H. Jenny. Ueber Basenaustausch. (of Proceedings) [On base exchange]. First Int. Congr. of Soil Sc. Abstracts Commission II: 40-46, English translation, 46-51, French translation, 51-55. Washington, D.C. 1927.

13. H. Jenny, Bemerkungen zur Bodentypenkarte der Schweiz. Landw. Jahrbuch d. Schweiz, 379-383. 1928.

14. H. Jenny. Relation of climatic factors to the amount of nitrogen in soils. Jour. Amer. Soc. Agron. 20:900-912, 1928; Proc. Amer. Soil Survey Assoc. Bul. 9: 163-173. 1928.

15. H. Jenny. Soil investigations in the Swiss Alps. Proc. 1st Internatl. Congr. Soil Sci. 4:421-428. 1928.

16. H. Jenny. Relation of temperature to the amount of nitrogen in soils. Soil Sci. 27:169-188. 1929.

17. H. Jenny. Klima und Klimabodentypen in Europa und in den Vereinigten Staaten von Nordamerka. Bodenkundliche Forschungen, Bd. I. 139-189. 1929.

18. H. Jenny. Hochgebirgsboden. Blancks Handbuch der Bodenlehre, Band III:96-118. 1930

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23. H. Jenny. Soil organic matter-temperature relationship in the eastern United States. Soil Sci. 31:247-252. 1931.

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Hans Jenny
The Soil Resource
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New York - Heidelberg - Berlin.

Foreword: Hans Jenny and Fertile Soil

Hans Jenny has an unusual capacity for originating ideas, applying them and communicating them to others. In any field of knowledge, truly original ideas are rare, and particularly those which are pursued to the point of stimulating an entire generation of scholars in the field. Hans Jenny has done precisely that for soil science. Those who are or have been privileged to be his colleagues, students and friends are deeply grateful. (1)

PAUL R. DAY

The soil resource is supporting a growing world. Hans Jenny's fertile imagination has supported the science of origin of soils. Now it supports a broadening and deepening of the foundations of ecology.

Vignettes from Jenny's life story tell something of how these twin contributions came to pass. They illustrate how ideas and traditions can crossbreed with one another for hybrid vigor in the history of science. I hope they show a bit deeper insight into creativity than we can usually learn from leading architects of science. Historians of science can follow details in an *Oral History* of Jenny's life (7). The Editors of *Ecological Studies* and Springer-Verlag feel especially privileged to make available not only the present book as a major historic contribution to pedology and ecology, but also the following personal sidelights about how that contribution was created over a full lifetime of observations, analysis, and synthesis.

The second half of this book is a substantial extension of the natural philosophy that lit up the world in the classic *Factors of Soil Formation* in 1941 (5), a popular textbook, long out of print and pageworn by heavy use. Jenny's *factor–function paradigm*, or framework for organizing thought, is as lively as ever today. It seems likely to me to enjoy a still broader renaissance for the remainder of the twentieth century and for the twenty-first, as the world's people come to realize—and try harder to understand and reverse—our wasting of the rich and varied resources of soils and ecosystems now exploited ever more intensively.

The first half of the present book reaches far back to the foundations of many sciences. It shows concisely, in as self-contained a manner as possible, how these contribute to understanding the many processes that maintain our soil resource as an operating system—part of the larger ecosystems of various sizes.

The fundamental concept of balance or imbalance of forces, of energy budgets, of rates of mass income and loss, here emerge from physics and chemistry to give a view of changing soil (or changing ecosystems) in terms of quantitative rates. Where the change is not zero (the ideal balanced system, rarely found in Nature), the *rate* of

change is simply described by the rate of income and rate of loss. Our home's energy budget, our firm's inventory, our nation's debt, and humanity's numbers all have accounts that change at rates that are equal to the inputs minus the outputs. Jenny's "system view" of the soil was carried into the fertile fields of Midwestern American prairies from the laboratories of Switzerland in the late 1920s. Jenny's rate equations provided the other paradigm or world view that, I recall, brought us to the threshold of systems ecology as it later evolved in the second half of the twentieth century.

As if world renown in the specialties of pedology and soil chemistry were not enough for one lifetime, excerpts below remind us that Hans Jenny has also been a perceptive outdoor field ecologist since his early Alpine expeditions with Braun-Blanquet in the mid 1920s. Jenny's ecosystem studies in the pygmy forest, a further classic example of a soil–plant system "run down" over hundreds of thousands of years since its origin, continue to occupy some of the vigorous retirement time near his farm in Mendocino County. But each specific, quantitative case study, and each research area conserved (with additional hard work) for further study by future generations, fits into Jenny's coherent world view. It is that view, and its legacies of discovery and of tangible landscape preserves, which we are privileged to share with their originator in this volume. In his eighties, Hans Jenny has also remained active in relating some of his favorite research areas to our global ecosystem problem of carbon and nitrogen cycling.

Jenny's notes for his *Oral History* (7) and the personal knowledge I have enjoyed with Hans Jenny since my own semester pilgrimage to Berkeley in 1950, may therefore help many kinds of readers to find more of Jenny's own life story between the lines in the remainder of this book. Little Hans (born 1899) grew up mostly in Basel, with a student's sparkling view of the world from the extraordinarily powerful technical institute in Zurich, and later with a series of very productive positions in his adopted America. Typical of Hans's always fresh observations was a teen-age incident while on farming "duty" in the Swiss valleys in World War I. The boy from town was awed by the distant view of the "snow mountain"—but his host family had lived with it for years without even noticing. Hans still points out fresh views of a world we have lived in without fully seeing. He shares with us his special scientific perception, as well as his admiration for each view. The beauty lies not only in landscapes he describes here (reviewed from an artistic standpoint in his famous Vatican lecture on pedology and art) (6). The Platonic beauty of Jenny's ideas emerges from their elegant ways of connecting so many fragments of knowledge that might otherwise become lost in their various pigeonholes—if not forgotten altogether.

Each of Jenny's life sojourns brought new insights, which then hybridized with one another in a kind of cross-fertilizing of ideas. This hybrid vigor of definitions and hypotheses was then subject to severe natural selection, a "survival of the fittest" theory, to make sense of the world. That selection involved the balance in a system of ideas: the fresh mutations of the intellectual world. Whence came this originality? the cross-breeding? the survival of two paradigms?

Georg Wiegner, Jenny's mentor at Zurich (7), was the Professor of Agricultural Chemistry, a physical chemist who motivated and guided Jenny's early work on ion exchange. (That continuing interest is reflected later in the present book.) Jenny's famous syllabus on colloid chemistry (4) not only reflects this individual interest, but

the power of the giants of physical science whom Hans could see and hear around Zurich. Yet Wiegner opened Hans's eyes to the fresh view of Hilgard from Berkeley and Dokuchaev from Russia, still unconventional for European agriculture of the 1920s. With more field experience than his mentor had time for, as in his vacation excursions collecting samples for analysis, Hans soon gained the precocious power to make connections that none of his predecessors was equipped for. The early "hybrid" book on soil acidification and successions (3) promptly gained worldwide attention in the Fuller/Conard English edition of Braun-Blanquet's *Plant Sociology* (2).

> When Tüxen in the 1950s praised the little book as a classic, I replied laughingly that it was a Jugendsünde (an illegitimate product of youth). In one of the sections I drew a speculative curve relating humus to climate, relying on some of Hilgard's analyses. It was a premature embryo, but nevertheless the concept of a climofunction was clearly stated. . . . . I never dreamed that a decade later I would sit in his chair in Berkeley, California, and teach his course on soil formation.
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> When I expressed a desire to study plant nutrition with Professor Hoagland in California I was politely told to forget it and select a place nearer the Atlantic Coast [by a visitor named A. R. Mann when he probed Jenny's interest in an International Education Board Fellowship from the Rockefeller Foundation] . . .
>
> Not long after Mann's proposal, Professor S. A. Waksman, the soil microbiologist from the University of New Jersey (Rutgers) visited our group and urged me to come to New Brunswick and work in his lab on colloid chemistry of humus. The prospect of seeing the United States was exciting. . . . I landed one evening in the fall of 1926 in New York. . . . While Wiegner's physiochemical lab was spick and span with ultraclean benches and mirror-like floors, Waksman's biological lab in the basement of an old building was messy and required galoshes whenever it rained. As an interlude, I switched to Professor Shives's greenhouse, working on effects of ions on transpiration by barley plants. Years later I realized that lab lifestyles are not crucial, for it was Waksman who got a Nobel Prize, not Wiegner. . . .
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> In the summer of 1927 at Washington, D.C., the first International Congress of Soil Science was ably organized by Lipman, Director of the New Jersey Agricultural Experiment Station. Wiegner lectured on my Ph.D. thesis work on ion exchange and I reported on my Alpine soil analyses. I was thrilled to get to know in person the great soil scientists of the era. . . . A few months later the great Marbut reviewed the Congress and remarked that the paper by Wiegner and Jenny was probably the best, but that it wasn't soil science. . . . I had been taught that ion exchange was the heart of soil science. Later, at Missouri, I got to know Marbut better and appreciated his credo that there is more to soil science than colloid chemistry.
>
> For a grand, transcontinental tour to the West Coast and back, an entire Pullman train was chartered. . . . When the train came to an official halt, owner-driven cars were waiting and took us to the sites. . . . Many of us fell asleep during the afternoon tours because we had been arguing too long the night before. . . . The tour was a thrill and opened a new world. The red soils of the South and black soils of Canada were showcases of the climatic theory of soil formation. On the trip, Bradfield of Missouri decided to spend a sabbatical year with Wiegner in Zurich and he offered me his lab at Columbia, Missouri, an opportunity I grabbed with both hands.
>
> Now, I was an instructor in the Department of Soils, a combination that was still a novelty to me. In central Europe soil science was taught by the professor of agricultural chemistry and the scope of the lectures reflected the docent's personal, often one-sided, interests. Here, an entire staff dealt with the extensive domain of soil science and did it on a broader scale than what I had seen at New Brunswick. I attended Professor M. F. Miller's lectures on soil classification which centered on the soon to be abandoned system of soil provinces. Pedologist Professor H. H. Krusekopf took me along on his many trips to the loessial prairies and the forested Ozarks and taught me soil survey which then still relied on the plane table. To satisfy

my curiosity about land and people I cross-examined and pumped him relentlessly in field and office. Soon he became my pedological guru and a personal friend. Before long I teamed up with broad-visioned Professor W. A. Albrecht, microbiologist, and grew in the greenhouse soybean plants on mixtures of sand, and Ca, H-clay.

The tumultuous year of 1927 culminated in the presentation of my first nitrogen paper at the meeting of the intellectually vigorous American Soil Survey Association held at Chicago on November 5. . . .

Although Columbia, Missouri, lacked the international reputation of Zurich, it was in turn spared the exclusive cliques of prima donnas of science. Instead, the university's stimulating faculty fostered intercommunication. I was close to Brody (animal physiology), Kirby-Miller (philosophy), Robbins (botany), Schlundt (chemistry), Stadler (genetics), and Stearn (physical chemistry). The favorable environment encouraged my professional growth. It proceeded along the lines of pedology and soil colloid chemistry.

Pedologic Work

Still fascinated by the summer's transcontinental experience I kept pondering about the contrast between the dark-colored Canadian prairie soils and the lighter-colored counterparts in Missouri. From bulletins and unpublished reports I gathered soil nitrogen and carbon analyses and arranged them in tables and graphs. Within a few weeks it became clear that from Canada to Louisiana along a temperature transect having similar moisture conditions soil organic matter was sharply declining in exponential fashion. Well-wishers encouraged me to present the findings at the upcoming meeting in November in Chicago, which I did, as already mentioned. Most everybody seemed impressed, especially Marbut who became a friendly counselor. The response spurred me on to search for additional, supportive N-T-functions.

Russians had long known that in their country humus is linked to climate, a trend they attributed to moisutre stress because cool, wet countries dominate their North and hot deserts their South. The transcontinental tour taught me that in the Great Plains area temperature and moisture trends are not synchronic, as in Russia, but intersect at nearly right angles, which provides an ideal climatic checker board. I made the most of Nature's fortunate American design.

Climatic comparisons of Europe and the United States suggested that the famous Russian Chernozem, a soil rich in humus and having a calcium carbonate horizon in the subsoil, might also have arisen in central Kansas and Nebraska. I was longing to meet this idol of a climatic soil profile, and on a field trip to western Missouri I talked my graduate student companion (Roy Hockensmith) into driving the old model-T Ford westward deep into Kansas, at the 25 miles per hour it was capable of doing. We found the missing link and were elated, having seemingly confirmed the climatic principle of soil genesis. Alas, back in Columbia we were lambasted for having taken a state car across the state line without prior permission.

Eventually, I acquired a substantial collection of soils across the Great Plains along a transect of increasing mean annual precipitation but constant, mean annual temperature. Soil organic matter responded to rainfall logarithmically. The individual moisture and temperature relations enabled me to formulate an "equation of state," a three-dimensional N-climate surface, which was published in 1930 in the Journal of Physical Chemistry. I enjoyed seeing field data aligned by equations and derived aesthetic pleasure from the shapes of the curves. Several pedologists, however, accused me of trying to be erudite.

In the same year I wrote an extensive overview for *Research Bulletin* **152.** During the absence of the department head (M. F. Miller) in Europe, I smuggled in a discourse on the low corn yields in the South, attributing them to the scarcity of nitrogen rather than to summer dry spells, as was customary. Miller thought the thesis too speculative. Two decades later the idea was given credit for having helped foster the spectacular rise in corn yields that followed on the heels of advances in hybrid corn genetics. For my friends abroad I wrote an account in German in the "*Naturwissenschaften*" but Wiegner and his staff kept mum.

By now I was an assistant professor at $2300 a year. In 1931 I was awarded the Nitrogen Research Award of the American Society of Agronomy.

Meanwhile, the stock market crash of 1929 initiated economic upheaval that engulfed the

university. Young faculty members were laid off. I was spared in spite of complaints that the College of Agriculture favored a foreigner over native sons. Fortunately, the emerging Soil Conservation Service (SCS) absorbed many of the department's unemployed. The new adventure was organized by soil surveyor H. H. Bennett, and Missouri received special consideration because years before, F. L. Duley and M. F. Miller had established the first, systematic soil erosion experiments. The SCS was to occupy me profoundly at a much later date.

One day Professor Miller asked me to examine the effect of agricultural cultivation on contents of soil organic matter. I don't remember what prompted the inquiry; it put me on the trail leading to American soil history. Along old Highway 40 we selected the grazed but otherwise virgin Tucker prairie and adjacent cultivated land, both on level Putnam clay pan soil. We found that 60 years of farming had induced a loss of 35% of total soil nitrogen and a slightly higher amount of carbon. Additional comparisons discovered in the literature permitted the construction of a declining nitrogen-time curve (*Bull.* **324**, 1933). Years later I developed a simple mathematical model of gains and losses of N and C that envisioned an equilibrium or steady-state condition of soil organic matter.

Further work disclosed the secondary role of vegetation, topography, and parent material, telling me that I had "rediscovered" the five soil-forming factors of Hilgard and of the Russian Dokuchaev school. Still, the nitrogen-climate surface qualifies as an original contribution to the storehouse of pedologic knowledge. The logical extension of the factor scheme to the clan of pedogenic functions did not emerge until the subsequent California position enhanced my self confidence. . . .

Pedologist at Berkeley

When the Department of Plant Nutrition became my abode in 1936, it was already famous for its work on metabolic uptake and for establishing the existence of micronutrients. . . . In 1939 Professor C. F. Shaw unexpectedly died of a heart attack. The Shaw hiatus was resolved by creating at Berkeley a Department of Soils (1940) with Kelley as head, joined by Weir, Bodman, and the entire soil survey staff. In addition, Jenny and Overstreet left Plant Nutrition and became members of the new department, all of them located in Hilgard Hall. I was assigned to teaching a course in pedology, following Shaw, and to bringing Dean Lipman Hilgard's roll-up desk. However, I continued teaching the colloid course.

Struggling with Definitions

I felt an urgent need to formulate a conceptual scheme that would embrace climate and time functions and others as well—in short, a logical theory of soil-forming factors. For over a year I pondered intensely about the meaning of soil formers, and what I read in the literature about them appeared logically inconsistent and often indulging in circular reasoning.

Why did a physical chemist choose temperature and pressure as crucial variables? What puts time and climate into the same category? I was led to conclude that soil-forming factors were neither causes nor forces, as claimed, but factors that define the state and history of a soil. The immense variations that exist among soils on the Earth were variations within a collection or ensemble of properties. Moreover, the factors could vary independently, whereas many properties could not. The independency criterion was widely misunderstood. It earned me such attributes as being deceitful and naive.

The meaning of parental material [the French and Germans speak of "mother rock"] kept me in suspense for quite some time. Finally, I pinpointed it as the initial stage of a soil at soil formation.

The real bugbear was the biotic factor. Like everybody else, I could see that vegetation affects the soil and that soil affects the vegetation, the very *circulus vitiosus* that I was trying to avoid. It took me a while to abandon "vegetation" and substitute "flora," eventually thinking of "flora of influx." Yet, in *Factors of Soil Formation* I was not fully consistent. Years later, prolonged discussions with R. L. Crocker, Jack Major, and N. C. W. Beadle confirmed the central core of thought.

Factors of Soil Formation

As far as I could tell, no pedologist has been digging for definitions more intensely than I. What is of interest to me, I reasoned, might interest other pedologists as well. I decided to write a book about concepts, supported by whatever evidence I could put my hands on.

The book is based on the soil-forming factor equation, dubbed by students as "*clorpt.*" On the left side are the soil properties s, on the right side the factors climate (cl), organisms (o), topography (r), parent material (p), and time (t). Soil-forming factor equations were nothing new, as I learned subsequently. They try to fuse properties and factors into a conceptual scheme. But by redefining the factors as possible independent variables, *I could solve the equation. That was the new approach.*

I submitted the manuscript to McGraw-Hill, whose editor, a botanist, turned it down. Again, R. Bradfield rescued it. The reactions were mixed. Overstreet said; "It's the kind of book I like." Thorpe didn't like it at first, then changed his mind, he told me.

Interest in the Ecosystem

The concern with soil nitrogen and soil carbon stemmed from considerations of soil fertility. The two elements enter the soil along biological pathways, hence vegetation is important in soil genesis. In the 1941 book, *I saw soil and vegetation as coupled systems* and designated the two together as the "larger system." Crocker then called my attention to Tansley's term of *ecosystem*. The letter l in the equations goes back to the "larger system." I became curious whether in a forest the vegetation contains more or less C and N than the soil. At Hobergs, California, in collaboration with A. Schultz, H. Biswell, J.R. Sweeney, and R. Glauser, we felled three trees, dug up the roots and collected the soil quantitatively. Analytical work and calculations dragged on for years, and, finally, a brief summary appears in this book.

To appraise the build-up of C and N in an entire ecosystem, my wife (Jean) and I spent three summers (1955, 1956, 1957) in the Swiss Alps collecting over 30 soil and vegetation (shrubs) tesseras on historically dated moraines of the Rhoneglacier. It took one-half to a whole day to collect a tessera in the rocky drift. In spite of cold and rainy weather, it was an enjoyable experience. An account appeared in a German journal in 1965.

The full impact of a holistic ecosystem view on soil genesis manifested itself in collaboration with R. J. Arkley and A. M. Schultz working on the Pygmy Forest Ecological Staircase near Mendocino, California. We had to answer the basic question whether the Spodosol created the pygmy forest, or the pygmy forest the Spodosol (7).

For more, various readers may find quite different paths through this book. After an overview of Contents, chapter summaries, and many of the pictures, a sequential reading straight through could construct the soundest foundations on which Jenny erects his building of ideas and equations. Sections in finer print—like closets that are opened later to reveal a storehouse of additional, useful details—can be skipped while first touring the overall structure and making ourselves more "at home" in the book. But the hallways and staircases connecting different levels of abstraction can be explored and then retraced in several orders, each pathway making more connections than our first guided tour.

The windows from this book looking out to the real world are its pictures. Many are by Jenny's own hand. To carry through the artistic touch inherent in this book and in Jenny's life (6), the editors have deliberately retained many of his original sketches that he expected to be redrawn or relettered to a more standardized, but less spontaneous style.

Like the "snow mountain," spied from afar by the farm youth; like each peak which the Alpinist Jenny continues to see as a challenge, the pinnacles of his career are reflected in the chapters and tables to follow.

Many other volumes of the *Ecological Studies* series contain material closely related to that of the present one (especially Volumes 1, 2, 4, 5, 6, 8, 10, 11, 15, 16, 17, 19, 26, 27, 29, 32, 34). Volume 35 by John Fortescue, on *Environmental Geochemistry: A Holistic Approach*, in particular can be viewed as a companion or complement to the present one. From the view of a geochemist exploring the periodic table as well as the factors controlling the landscape's state, Fortescue is explicit about the interdisciplinary character of geochemistry, ecology, and pedology; also about the paradigms which have been changing the character of each in recent decades. Now we have a fuller working out of the closely related paradigms, which Hans Jenny brings together in the present volume.

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Oak Ridge, Tennessee
September 1980

Jerry Olson

My friend, the soil

A conversation with Hans Jenny

Hans Jenny, no stranger among soil scientists, has begun to receive wider public recognition for his many professional achievements. Last year, SCSA presented Jenny, an SCSA member, its Honor Award. More recently, the book accompanying a new British Broadcasting Company television series, "Bellamy's New World," was dedicated to Jenny. Then in February of this year, a symposium was held at the University of California—Berkeley to commemorate Jenny's 85th birthday and to raise funds for the Hans Jenny Land Conservation Fund, which is used to acquire land chosen by Jenny for its value to soil appreciation and study.

A native of Switzerland, Jenny came to the United States in 1926 as a Rockefeller Fellow. He joined the University of Missouri—Columbia faculty in 1927 and moved to the University of California—Berkeley in 1936. Jenny's early research produced a new, quantitative description of soil genesis. While at Missouri, his work took a practical bent, focusing on problems of declining soil fertility in the Midwest. His well-known text, Factors of Soil Formation, *was published in 1941.*

A colleague has written of Jenny's research career: "In a field of knowledge, truly original ideas are rare, and particularly those which are pursued to the point of stimulating an entire generation of scholars in the field."

Though his career pushed into numerous technical frontiers, Jenny's interests also demonstrated a broad, imaginative concern for the soil. In this interview, he discusses his view of soil as a thing of beauty—its profound part in art history, its dynamic ecological "life"—articulating a view of soil that offers a more exciting, creative perspective for soil conservationists and laymen alike.

Jenny was interviewed by Kevin Stuart, a graduate student at the University of Hawaii.

■ **KEVIN STUART:** *Would you tell me what the basis is for your appreciation of the aesthetics of soil?*

■ **HANS JENNY:** I am surprised; how did you know of my interest in soil art? Well, soil appeals to my senses. I like to dig in it and work it with my hands. I enjoy doing the soil texture feel test with my fingers or kneading a clay soil, which is a short step from ceramics or sculpture.

Soil has a pleasant smell. I like to sit on bare, sun-drenched ground and take in the fragrance of soil. As yet, neither touch nor smell sensations have been accorded aesthetic recognition, but colors delight painters, photographers, and writers, as well as you and me.

In loess country, plowed fields on slopes show wide bands of attractive color gradations from dark browns to light yellows, caused by erosion of the surface soil. Warm brownish colors characterize fields and roofs in Cézanne's landscape paintings of southern France, and radiant red soils of the tropics dominate canvasses of Gauguin and Portinari. Soil profiles viewed in pits may reveal vivid color and structure patterns of layers or horizons. I have seen so many delicate shapes, forms, and colors in soil profiles that, to me, soils are beautiful.

Whenever I offer this reaction to an audience, I notice smiles and curiosity, but when I follow up with slides that depict ebony black mollisols of Canada, titian-red oxisols of Hawaii, and gorgeous soil profile paintings by such famous artists as Grant Wood of Iowa, Dubuffet of France, and Schmidt-Rottluff of Germany, the hesitancy turns into applause.

■ **STUART:** *How would you explain the lack of aesthetic appreciation of soil on the part of many soil scientists?*

■ **JENNY:** I don't know. Maybe they lacked early exposure to art appreciation. My grandfather was a wood carver, and his sons and daughters kept an interest in art. In high school I had an art teacher who took us regularly to current art exhibits in local galleries and museums. Soil profile art is not akin to classic paintings with themes; rather, it resembles abstract art; and if you are used to thinking of soil as dirt, which is customary in our society, you are not keyed to find beauty in it.

■ **STUART:** *Why talk about soil imponderables, like beauty, and should this be an important aspect of soil science?*

■ **JENNY:** Confronting an exposed soil cut may be an exciting event. Soil speaks to us through the colors and sculptures of its profile, thereby revealing its personality; we acknowledge it by giving soil a name, albeit in a foreign tongue, but we don't mention our emotional involvements. In fact, our soil language is lifeless, and the soil descriptions in our publications are utterly boring to the farmers, ranchers, foresters, sportsmen, and newcomers who are sup-

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posed to read them. Articulation would strengthen our feelings about the soil body. Casually and in formal lectures, we may want to talk more openly about soils and do it more enthusiastically. We may even become more interesting persons. We may gain new friends, and they might hold a positive opinion of the soil resource.

■ **STUART:** *How important do you think soil science and soil scientists are in influencing the rest of the society's ideas about soils?*

■ **JENNY:** Quite unimportant, I'm afraid. Our technical articles are being read by a small coterie of fellow specialists, and the leading national press ignores them completely. In its heydays of the 1930s and 1940s, the Soil Conservation Service electrified the nation and got the school children involved, but those days are over.

Some months ago the *Christian Science Monitor* published a lengthy discourse on soil deterioration, but I could not detect an echo in the city press. Perhaps as long as supermarkets are well stocked with food, the city dweller does not look beyond. It is a shame that the many excellent and interesting papers by soil scientists remain confined to library shelves. New findings by chemists, geologists, geneticists, and ecologists are regularly reported by the media, and commented upon, but the subject of soils seems to be taboo. I suspect that our intellectual isolation and our invisibility have to do with the lack of formulating exciting ideas about soils themselves and their relations to people, and the shortage of popularizing soil science writers.

I am tremendously interested in what the public reaction will be to "Bellamy's New World," now being shown abroad by the British Broadcasting Corporation. In an eight-part television series and in an accompanying book, David Bellamy offers a remarkable botanical history of America in which he stresses the significance of soils for plants and men, and describes, with charming enthusiasm, important soil profiles.

■ **STUART:** *How would you describe the idea of soil as interpreted by the discipline of soil science?*

■ **JENNY:** To my famous teacher G. Wiegner, soil was an object to apply known principles of colloid science and to discover new ones. He expressed little interest in the contract of humans with soil. My former colleague Richard Bradfield studied soils in the laboratory and field with the aim of helping farmers man-

***Arbor Day* by Grant Wood**

ge their soils and provide food for humanly in general. That's why he worked for ears on international agricultural projects ith the Rockefeller Foundation.

A soil creed that began to spread in the 920s states that soil is a *natural body* that eserves scientific study and contemplaon, as is accorded other natural bodies, he organisms, rocks, oceans, and stars. his formulation marked a radical change rom viewing soil as merely a cog in the gricultural production machine, yet the reed has benefitted farming in many imortant ways.

My own approach to soils has changed everal times. In my younger years, with ny farm background, whenever I thought f "soil" I visualized a plowed field. You eed a crop and what yield you get depends great deal on the nature of the soil. ater, during my Missouri work on soil umus contents, I soon realized that the rime source of organic carbon in soils reides in the organic matter furnished by ree litter fall, grass mats, crop residues, nd root production. In California, I got nvolved with pristine and near-pristine rasslands and forests and began thinking in terms of the "larger system" that is composed of *soil* plus *vegetation* plus *animal life*, a combination that is now known as ecosystem. Hence, I see soil in the context of a living, dynamic ecosystem, either a natural, or an agricultural, or a silvicultural one.

■ **STUART:** *You talk about living systems. Why is it important to you to include this aspect?*

■ **JENNY:** Many ecologists glibly designate soil as the abiotic environment of plants, a phrase that gives me the creeps. Is the bark of a tree the abiotic environment of the tree? And what about the bacteria-rich rhizosphere? Looking at the root-soil boundary under the powerful electron microscope, an observer cannot tell where the biotic part ends and the abiotic part begins.

Soils contain over a thousand different species of lower animals, the earthworms, pill bugs, nematodes, millipeds, termites, ants, springtails, and amoebas, not to mention the millions of molds and bacteria. My late teacher, Professor S. A. Waksman, discovered in soils the microbes that produce the antibiotic streptomycin that cures tuberculosis; he, who signed his letters as "soil microbiologist," was awarded the Nobel Prize in medicine. When I add up the live weights, exclusive of roots, estimated by soil biologists, I find more living biomass below ground than above it, amounting to the equivalent of 12 horses per acre. The soil organisms consume oxygen from the soil air and give off carbon dioxide, and the summation of the multitudes of respirations characterize the metabolism of a soil individual. Hence, I designate soil as a living system. Bellamy talks about the living soil. It is, however, not an organism because soil does not multiply; you don't find two masses of soil where there was one.

If all the elephants in Africa were shot, we would barely notice it, but if the nitrogen-fixing bacteria in the soil, or the nitrifiers, were eliminated, most of us would not survive for long because the soil could no longer support us. I can't help thinking of the claim that healthy soils make healthy people, and as an extension, I am intrigued by the thought that good soils make good people, but that notion seems untenable. Well, not wholly so. Working in the garden with spade and hoe soothes the minds of many people.

■ **STUART:** *You described how Wiegner and Bradfield looked at the soil, but I wasn't sure where you placed yourself.*

■ **JENNY:** Observing soils, studying them, and reflecting on them induces respect, if not wonder. All of us relate to soil unconsciously in our daily nourishments that make us participants in the continuous flow of nutrient atoms that originate in the soil. And in the final act our bodies are returned to the soil.

Over the years I have acquired a kind of reverence for the soil, for the creature-world inside it, and for its character expressed in the profile features. Where big logging equipment turns soil upside down to make earthbeds for falling redwood trees, the mass of soil remains at the site and no "environmental damage" is said to occur. Yet the soil profile, the soil's signature and identity, is obliterated. Though I consider such profile destruction an irretrievable loss, I have never seen anybody shedding tears about it. My attitude may be a personal quirk, or a result of lifelong interest in soil. In the latter case, I might not be alone. Whatever, I am glad I feel the way I do.

■ **STUART:** *Has this train of thought led to your actions for preserving virgin soils and their landscapes?*

■ **JENNY:** Reverence for the soil has turned me into a preservation advocate of natural, undisturbed soil, regardless of whether it is a good soil or a poor one, or a rare and endangered soil species or not. My wife, friends, and well-wishers helped pre-

Saarland Museum

Northern landscape by Karl Schmitt-Rottluff. Done in present-day East Germany, this painting emphasizes soil horizons of the podzolization process.

serve several soils in California: the pygmy forest ecological staircase (Jug Handle State Reserve), which is an age-sequence of marine terraces and their old soils; the Mt. Shasta mudflow area, composed of incipient ecosystems; Apricum Hill with a fossil laterite crust; and Jepson vernal pool prairie, held by The Nature Conservancy.

Other sites are still under consideration. These natural areas were set aside as benchmarks for assessing man-induced soil changes and for preserving unique segments of landscapes that possess pedological and ecological potential for teaching and research. While reverence for soil was not explicitly invoked, it helped sustain the efforts to secure these lands.

Society grants human beings the right to exist, regardless of whether we are useful or not, and that right cannot be abrogated without due process of law. The same privilege has been extended to a few endangered plant and animal species. I wish society would grant the same right to soil. The prospects are bleak.

■ **STUART:** *Does the soil have a right to be protected for any reason other than that based on what is best for humans? For example, if gold is of value to our society, then why not dig for it and in the process destroy a fertile, producing soil and perhaps even push its remains into the sea?*

■ **JENNY:** Your case is not fictitious. Extensive areas of good soils have been demolished during extraction of their minute particles of gold. The way you phrase the question brings up nature's edict that we humans cannot live without sacrificing plant and animal lives, and that land must be cleared for growing crops and building habitations if we do not want to live as nomads.

Today, the idea of stewardship of land is pitted against the belief in soil exploitation for personal gain and that soil is merely an economic commodity in the marketplace. And who decides "what is best for humans"? I place natural soils and ecosystems, the nature museums, on par with art museums, automobile and railroad museums, golf courses, racetracks, music halls and gambling halls, even colleges and temples. They all use up space and appeal to special groups of people who are not called "elitist," and all these places get public financial support and protection in one form or another.

■ **STUART:** *As a student of the sciences, how did you get interested in soil?*

■ **JENNY:** It was the other way around. I had an interest in soil and to comprehend it I needed science.

Grassland soil in California. Mellow ambers and browns of subsoil are enlivened by shadows of peds and aggregates.

In my research I tried to learn how soils are formed, how nature creates soils, and how long it takes to make a soil. In such studies investigators locate soils of known ages that may extend over decades, centuries, and milleniums. They analyze the soils; arrange the data according to soil ages, which yields "time sequences" of soils; and deduce how fast the soil bodies are changing.

One question looms large: What does nature have in mind, what is her goal of soil evolution? For the animal kingdom, evolution is said to improve the design of organisms, leading to higher, more complicated beings, with humankind at the apex.

■ **STUART:** *What plan would you say nature has for soil?*

■ **JENNY:** On soft rocks the several dozens of time sequences so far on hand suggest that in high rainfall regions and in the absence of catastrophes, such as earthquakes and severe erosion episodes, the production of organic matter as biomass rises from near zero at the beginning of the sequence to a maximum in a few thousand years, and then very slowly declines because the endless water infiltration under high rainfalls leaches out the nutrients of the soil, raises the acidity, and may establish hardpan and claypan horizons that curtail root growth. Often these areas harbor unusual plants and animals that grow nowhere else (endemics), and such soils may play a role in the evolution of species.

■ **STUART:** *Is your query on what nature has in mind for soils a legitimate question, and if it is, why shouldn't we also ask about the purpose nature has for an entire ecosystem?*

■ **JENNY:** You might go a step further and ask what is the purpose of all nature? And if you include ourselves as a part of nature, what is our role on this earth? Soil science, or any other science, cannot provide an answer, only religion or mysticism might do that. □

Mountain soil beckons UC prof

by JANET KORNBLUM
STAFF WRITER

Most people climb Mt. Kilimanjaro "because it's here." Professor Emeritus Hans Jenny climbed it because of what's there: soil . . . thick, black, rich soil that he always believed was there; but he could never get a grant to make a safari to Africa. So two years ago, at 83, after 46 years of teaching and studying on the Berkeley campus, he and an assistant took off to fulfill a lifelong dream. Jenny paid for the expedition out of his own pocket.

Jenny found what he was looking for: humus soil, which is high in organic content and makes better planting soil for food production. His find could mean a lot to farmers because humus helps prevent harmful erosion.

Jenny concerns himself with all soil-related issues and problems, for he is a soil expert "from the atom to the plow."

These days the professor works more on the plow end. When he retired from teaching in 1969, Jenny left his ivory tower and encountered a world of "interested and concerned people" he never knew existed. He went outside and learned the language and ideas of supervisors, planners, foresters, farmers and politicians.

He has helped preserve three areas valuable for scientific research of natural soil formations. One of these is the Pygmy Forest Staircase in the Jug Handle Reserve in Mendocino, a unique site containing well-preserved soils from different evolutionary periods. At the top lies an area of trees which are dwarfed due to lack of nutrients in the ancient soil.

SEE PAGE 19

The state's acquisition of this forest occured during Ronald Reagan's term as governor. "When Reagan was governor of California, he had a director of natural resources who was sympathetic to preserving significant natural areas," Jenny says. "But on the national level, (former) Secretary James) Watt has the opposite view.

"Environmentalism now is low-key, but it may change again. Too many people are concerned about the environment." He states in a matter-of-fact tone coming from long years of scientific rationalism that he is currently trying to preserve an area of red soils: "I'm in competition with a rifle club that wants a rifle area and a parking place for 1,000 cars. The problem of scientific interest becomes a political problem."

Although he does not directly involve himself in politics, he has attended conferences at the Department of Agriculture to relay his ideas about how the United States and the world could improve crop production.

Jenny says farmers should be made to improve the organic content of the soil they work. Jenny said private gardeners have no problems doing this, as they keep up with modern soil improvement methods such as chemical fertilization. But farmers who own large pieces of land are not as willing to improve their soil because of the high costs.

Farmers set themselves back by letting their land erode — a problem that could be dealt with by educating farmers. But it's not that simple. "It's like educating people not to smoke. On every advertisement there's something written about how bad it is, but people smoke anyhow. Education is not the answer. It should be related to payments in agricultural programs. It should become a policy of the Department of Agriculture to improve the organic matter of the soil."

Jenny's enthusiasm for his work seems unending. He first became interested in the soil as a young boy living in the Swiss Alps. "I was raised on a farm where I plowed soil, grew crops and developed an unconscious relationship with the soil," he said in distinctly enunciated words reverberating with a faint German accent.

He later attended the Federal Technical Institute in Zurich, where he was influenced by a professor "who had new ideas about soil." After Jenny received his doctorate in soil chemistry, a representative of the Rockefeller Foundation invited him to spend one year in the United States. He came in 1927, and has been here ever since.

Jenny sees himself as continuing Professor Eugene W. Hilgard's work in soil biology. "When I was a graduate student I studied Hilgard's book on soil, but I never dreamed that ten years later (1936) I would be sitting at his old desk in Hilgard Hall teaching his old class. The question is: is this a coincidence or are the forces at work?"

Jenny is perhaps best known for his classic 1941 text *Factors of Soil Formation*. His most recent text, *The Soil Resource*, was published in 1980.

When not writing or climbing mountains, Jenny spends time with his wife and family at his farm in Mendocino.

To celebrate Jenny's 85th birthday, his colleagues have organized an all-day symposium on Saturday, for which speakers have flown in from across the United States. The symposium, which is free and open to the public, will begin at 9:00 a.m. in 101 Morgan Hall and will end at 4:30.

Both honored and flattered by the symposium, the professor said, "What surprises me is what people think of me and what I think of myself. It doesn't jibe." When asked what he thinks of himself he said, "I'm an interesting guy with a lot of ideas but I'm not sure how important I am. I see so many inadequacies. For years students have said how good I am, but you don't know. Maybe they wanted a better grade."

Gazette

CalReport / January 1987

Hans Jenny, professor emeritus, Plant and Soil Biology, has been elected an honorary member of the International Society of Soil Science.

Also, the recent book titled *Bellamy's New World* has been dedicated to Jenny. The book by British botanist David Bellamy is a companion to his BBC-TV series on the botany of America—which includes an episode on Jenny's studies of the Pygmy Forest in Mendocino County.

The Hans Jenny Land Conservation Fund has been established in honor of Dr. Jenny's contribution to understanding the relationship between soils, climate, and plants. This fund will be used to acquire land chosen by Dr. Jenny to illustrate soil relationships. The Nature Conservancy has been chosen to administer the purchase of the selected land. A gift to The Nature Conservancy earmarked for this fund will honor Dr. Jenny on the occasion of his 85th birthday and will serve as an enduring tribute to his life's work.

The Nature Conservancy
156 Second Street
San Francisco, CA 94105

The Family and Friends of

HANS JENNY

invite you to a reception

in honor of his 85th

birthday

Sunday, February 12th, 1984

4 p.m.

at the Faculty Club,

University of California,

Berkeley, California

The party is a surprise.

No presents please.

RSVP

You are cordially invited to
celebrate the 90th birthday of

Professor Hans Jenny

Sunday, March 12, 1989
Haas Clubhouse, UC Campus
Festivities will begin at 2PM.

Join us in this celebration in recognition of this great man and friend!

Professor and Mrs. Jenny request that no gifts be brought. Instead they suggest that if anyone wants to do something for Professor Jenny, a donation to the Nature Conservancy (San Francisco) is always very helpful. Through such donations, they have purchased about 330 acres of pygmy forest. The Nature Conservancy is very good about communicating with Professor Jenny regarding such donations and always thanks the donor.

To RSVP please call and leave a message at 945-1234, For further information, please contact Professor Paul Gersper at 642-3005 or Andra Strads at 642-2210.

The Department of Plant and Soil Biology

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Interviews on the History of The University of California

Documenting the history of the University of California has been a responsibility of the Regional Oral History Office since the Office was established in 1954. Oral history memoirs with University-related persons are listed below. They have been underwritten by the U.C. Berkeley Foundation, the Chancellor's Office, University departments, or by extramural funding for special projects. The oral histories, tapes and transcripts, are open to scholarly use in The Bancroft Library. Bound, indexed copies of the transcripts are available at cost to manuscript libraries.

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Wage Rate Analyst with the Twelfth Regional War Labor Board, 1943-1945, specializing in agriculture and services. Research and writing in the New York public relations firm of Edward L. Bernays, 1946-1947, and research and statistics for the Oakland Area Community Chest and Council of Social Agencies 1948-1951.

Active in community affairs as a director and past president of the League of Women Voters of the Hayward Area specializing in state and local government; on county-wide committees in the field of mental health; on election campaign committees for school tax and bond measures, and candidates for school board and state legislature.

Employed in 1967 by the Regional Oral History Office interviewing in fields of agriculture and water resources. Project director, Suffragists Project, California Women Political Leaders Project, and Land-Use Planning Project, and the Kaiser Permanente Medical Care Program Project.